The SCIENTIFIC MONTHLY

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Desk

MEDELLIN, COLOMBIA
See *Agricultural Education in Colombia*, page 91.

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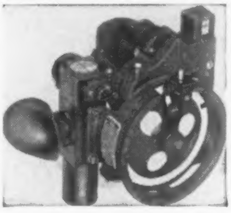

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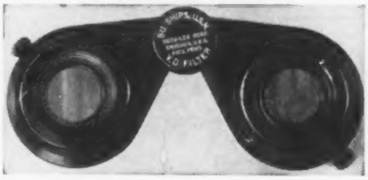

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New Books Received

The Years After 50. WINGATE JOHNSON. xii + 153 pp. $2.00. Whittlesey House. New York. 1947.

Caves of the Upper Gila and Hueco Areas in New Mexico and Texas. C. B. COSGROVE. xv + 182 pp. Illus. $6.25. Peabody Museum. Cambridge, Mass. 1947.

Rudiments of Chemistry. ROLAND M. WHITTAKER. x + 310 pp. Illus. $3.00. Ronald Press. New York. 1947.

Energy Unlimited. HARRY M. DAVIS. xiv + 273 pp. Illus. $4.00. Murray Hill Books. New York. 1947.

Land and Ocean Magnetic Observations, 1927–1944. W. F. WALLIS and J. W. GREEN. v + 243 pp. $1.25, paper; $1.75, cloth. Carnegie Institution. Washington. 1947.

Ionospheric Research at College, Alaska, July, 1941–June, 1946. S. L. SEATON, H. W. WELLS, and L. V. BERKNER. *Auroral Research at College, Alaska, 1941–1944.* S. L. SEATON and C. W. MALICH. vi + 397 pp. Illus. $1.85, paper; $2.35, cloth. Carnegie Institution. Washington. 1947.

What Is Life? J. B. S. HALDANE. x + 241 pp. $3.00. Boni & Gaer. New York. 1947.

Alexander Dallas Bache. MERLE M. ODGERS. vii + 223 pp. $2.75. Univ. of Penna. Press. Philadelphia. 1947.

Medical Aspects of Growing Old. A. T. TODD. 164 pp. $3.50. William Wood. Baltimore. 1946.

Health Instruction Yearbook, 1947. OLIVER E. BYRD, Ed. ix + 325 pp. $3.00. Stanford Univ. Press. Stanford, Calif. 1947.

The Scientific Paper. SAM F. TRELEASE. xii + 152 pp. Illus. $2.00. Williams & Wilkins. Baltimore. 1947.

Fishes of the Great Lakes Region. CARL L. HUBBS and KARL F. LAGLER. xi + 186 pp. Illus. Cranbrook Inst. of Science. Bull. No. 26. Bloomfield Hills, Mich. 1947.

Hormones and Horticulture. GEORGE S. AVERY, JR. and ELIZABETH B. JOHNSON, with RUTH M. ADDOMS and BETTY F. THOMSON. xi + 326 pp. Illus. $4.50. McGraw-Hill. New York. 1947.

Biochemistry for Medical Students. WILLIAM VEALE THORPE. (4th ed.) viii + 496 pp. Illus. $5.00. Williams & Wilkins. Baltimore. 1947.

Fish Ponds for the Farm. FRANK C. EDMINSTER. xii + 114 pp. Illus. $3.50. Scribner. New York. 1947.

Physical Science and Human Values. A SYMPOSIUM. vii + 181 pp. $3.00. Princeton Univ. Press. Princeton, N. J. 1947.

Intertonguing Marine and Nonmarine Upper Cretaceous Deposits of New Mexico, Arizona, and Southwestern Colorado. WILLIAM S. PIKE, JR. ix + 104 pp. Illus. Geol. Soc. New York. 1947.

The Royal Botanical Expedition to New Spain. HAROLD WILLIAM RICKETT. 86 pp. Illus. $2.50. Chronica Botanica. Waltham, Mass. Stechert Hafner. New York. 1947.

Insects and Human Welfare. CHARLES T. BRUES. (rev. ed.) xii + 154 pp. Illus. $2.50. Harvard Univ. Press. Cambridge, Mass. 1947.

Microscopic Anatomy of Vertebrates. JAMES I. KENDALL. (3rd ed.) 354 pp. Illus. $6.00. Lea & Febiger. Philadelphia. 1947.

Trees and Toadstools. M. C. RAYNER. x + 91 pp. Illus. $2.50. Rodale. Emmaus, Pa. 1947.

Liberia. CHARLES MORROW WILSON. 226 pp. Illus. $3.75. Sloane Associates. New York. 1947.

Tertiary Nautiloids of the Americas. A. K. MILLER. viii + 234 pp. Illus. Waverly Press. Baltimore, Md. 1947.

Scientists Starred, 1903–1943 in "American Men of Science." STEPHEN SARGENT VISHER. xxiii + 556 pp. $4.50. Johns Hopkins Press. Baltimore, Md. Oxford Univ. Press. London. 1947.

Sexual Behavior in the Human Male. ALFRED C. KINSEY, WARDELL B. POMEROY, and CLYDE E. MARTIN. xv + 804 pp. $6.50. Saunders. Philadelphia. 1948.

Nuclear Physics in Photographs. C. F. POWELL and G. P. S. OCCHIALINI. xi + 124 pp. Illus. $6.00. Oxford Univ. Press. New York. 1947.

Glacial Studies of the Pleistocene of North America. WILLIAM HERBERT HOBBS. xii + 109 pp. Illus. $2.00, paper; $3.00, cloth. Edwards. Ann Arbor, Mich. 1947.

Introduction to Modern Physics. (4th ed.) F. K. RICHTMYER and E. H. KENNARD. xvii + 749 pp. Illus. $4.00. McGraw-Hill. New York. 1947.

Drugs from Plants. T. I. WILLIAMS. 119 pp. Illus. 6s. Sigma Books. London. 1947.

The Electron Microscope. V. E. COSSLETT. 128 pp. Illus. 7s. 6d. Sigma Books. London. 1947.

THE

SCIENTIFIC MONTHLY

FEBRUARY 1948

AGRICULTURAL EDUCATION IN COLOMBIA

W. H. HODGE

Dr. Hodge (Ph.D., Harvard, 1941), associate professor of botany at the University of Massachusetts, has spent much of the past ten years in Latin America. In 1945–47 he served as visiting professor and head of the Department of Botany at the Facultad Nacional de Agronomía, Medellín, Colombia. From 1938 to 1941 he was a teaching fellow at Harvard's Gray Herbarium; during this time he made floristic studies in Cuba and in Dominica, British West Indies. He spent the years 1943–45 in Peru for the Office of Economic Warfare looking for new stands of Cinchona.

HIGHER agricultural education in Colombia is provided by two agricultural colleges, one located at Medellín in the Department (i.e., state) of Antioquia, and the other at Cali, in the Department of El Valle. The school at Cali, originally a state agricultural institution, has been nationalized only recently. Since its current enrollment is small, the school cannot compare as yet in importance with the older Medellín institution, the Facultad Nacional de Agronomía.

The latter school was founded in 1914 as the national agricultural college. In 1937, however, it was incorporated as a *facultad*, or "college," of the National University of Colombia (Universidad Nacional de Colombia), from which it receives its annual budget, derived from the Ministry of Education. The National University, like most Latin-American universities, has scattered branches, or faculties, not only throughout various sections of the capital city of Bogotá, but also as far away as Medellín, Colombia's second city. Recently, in a new ultramodern development, the National University has brought all its Bogotá branches together onto a beautifully landscaped, U. S.-style campus, the Ciudad Universitaria (University City), located on the outskirts of the capital. Two of its branches, the School of Mines and the School of Agriculture, cannot feasibly be moved; these are located in Medellín. There is logic behind this seeming isolation of the University's branches at Medellín, for the State of Antioquia, among other things, can boast of being Colombia's gold-mining center and one of the most important cattle-selling districts; besides, it is the heartland of the republic's best-known export crop, fine-grade coffee. Moreover, Medellín offers other advantages for an agricultural school, for, situated only one mile above sea level—but with cool mountain heights at her doorstep—she can offer better conditions for growing most types of demonstration crops, from temperate potatoes to tropical cacao. Bogotá and her much colder environs can scarcely support anything but a temperate type of agriculture.

The principal aim of the Facultad de Agronomía is to prepare men for a career in Colombian agriculture, but, in addition, re-

FACULTAD DE AGRONOMIA

search is carried on by members of the institution. Extension work such as we have in the United States, although initiated, has not been extensively developed as yet; of course requests for aid from farmers or other interested individuals are honored.

Administrative work at the Facultad is in the hands of a dean. General rules and regulations of the agricultural school naturally follow those of the mother-University and in fact are largely drawn up by the directive council of that institution, of which the deans of the various faculties are members. Any modifications of those rules judged necessary by the school at Medellín must eventually be voted upon by the council in Bogotá. The Facultad, in turn, has its own directive council made up of six faculty members composed of a president (the dean), a secretary, two professors elected as representatives for the faculty, and two professors elected for and by the student body.

At the present time the faculty is composed of approximately twenty-five members—not a large number, but in proportion to the small student body of approximately two hundred it is ample. Of the faculty group, only about half can be considered full-time professors, or research men, stationed at the college. The remainder are professors who, though full-time teachers, are attached to, or share their time with, one of the other local educational institutions such as the National School of Mines, the University of Antioquia, or its School of Chemistry, and handle certain lectures only at the Facultad de Agronomía. In such positions are the men who give course work in mathematics and related subjects, geology, agricultural economics, and the like. The majority of the professional men are themselves graduates of the Facultad or of one of the other branches of the National University and, as such, usually hold the equivalent of a college undergraduate degree. Several have undertaken postgraduate studies in the States. In fact, Colombians have deep respect and admiration for our agricultural teaching and techniques. They look to the United States for guidance and mutual co-operative aid, they use American textbooks in the agricultural sciences, they send promising students to us for further study, and they aid professional men to further their research by profitable visits to the United States. A number of their federal agricultural men have studied in our country as students financed by their own government.

As a further means of improving either local research or teaching practices, Colombia has sought continually to invite visiting professors for a stay at the Facultad in Medellín. Colombians seem to prefer that these visiting men be North Americans, but these wishes are hard to fulfill because of language difficulties—all too few scientists in the States have the necessary background in Spanish to be effective teachers in that Latin tongue. Nevertheless, the Facultad has managed to attract some half-dozen United States scientists in recent years, including specialists in genetics and improvement of corn, plant taxonomy and general botany, tropical agriculture, and animal husbandry. Visiting scientists have come from such institutions as Iowa State and Ohio State; the Universities of California, Massachusetts, Texas, and Notre Dame; and the U. S. Department of Agriculture. In addition to these scientists, Medellín has sought elsewhere in the Americas for agriculturalists, having had an irrigation specialist from Mexico and a professor of pomology from Cuba.

It is somewhat easier to attract fellow-Latins to the Facultad since for these men

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there is naturally no language difficulty and little salary differential. The average U. S. scientific worker can hardly afford to leave the security of his position and higher salary for the pay offered by an educational institution in Latin America. One of the means of accomplishing the interchange of professors has been the funds made available by the Division of Cultural Cooperation of our State Department. Such funds have been utilized by visiting scientists to augment the low salary schedule obtaining in many parts of the other Americas.

In comparison with salaries found in many Latin republics, the top salary of a full professor at Medellín is good, averaging about 8,000 pesos (approximately $5,000) per year. But living costs, if a person is to live "a la U.S.A.," are very high, especially now with postwar inflationary prices. Except for the comparatively poor financial return—when considered over a long period of years—many a North American scientist might be more than pleased to remain permanently in Colombia to pioneer in a scientific field that has boundless frontiers. And it is only by remaining for long periods that a visitor can bring about the greatest mutually beneficial results. Over short periods it is more desirable to send promising Colombian students or professional men to the States for special training.

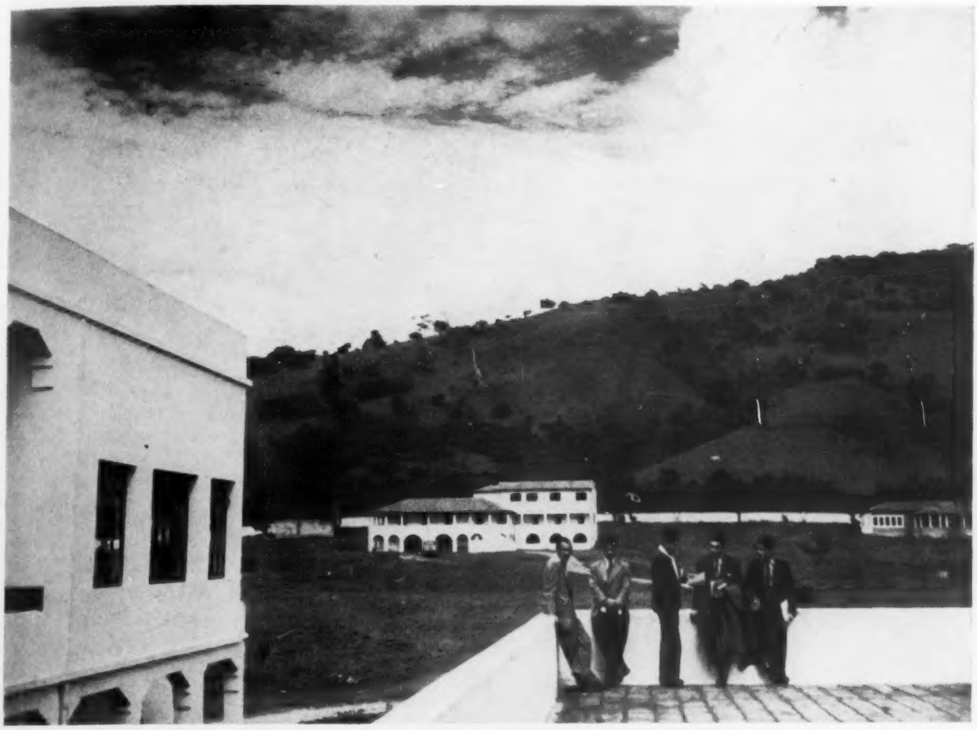
One of the basic reasons for Colombia's demand for agricultural specialists from North America is the inadequate number of her own agricultural corps. For, despite the fact that she currently graduates up to fifty potential workers a year—some of whom presumably could emerge as professionals —the fact remains that the better men are sooner or later lured, almost without exception, to higher-paid, more attractive commercial posts, often outside the agricultural field. There are jobs in agriculture, but because of the poor pay there are no men to fill them. Colombian government service, which can pay up to 1,000 pesos monthly for an engineer or a specialist in rural medicine, can offer a mere 350 pesos for a plant pathologist, or 280 pesos for an entomologist. (The Colombian peso is equivalent to 57 cents in U. S. currency.) Even the big commercial agricultural interests, such as the wealthy National Federation of Coffee Growers, who

control the production of Colombia's most important crop, pay very low salaries to their scientists. Obviously, Colombia must correct this situation, especially since agriculture is her No. 1 industry.

Admission to the Facultad de Agronomía is open to male students sixteen years of age or older who have satisfactorily completed their secondary-school education. These boys hold the Latin-American *bachillerato,* which is the equivalent of our high-school diploma. Although entrance examinations are required by the other schools of the National University, this is not true of the agricultural college, which unfortunately often gets a very poor class of student. In other words, most young men strive to enter one of the other branches of the University, where prestige is higher and the ultimate take-home salary will be greater. Thus the agricultural college often finds itself with an entering freshman class composed largely of students who have been turned down at the Medical School, the Law School, or the School of Mines.

This matter of prestige, which operates in our own schools to a minor extent, is a dominant feature of the Latin student's outlook. His background, like that of all Latins in the Central and South American republics, is one of arts and letters rather than the sciences. This tradition, of course, has its roots in the Iberian Peninsula. Spain's accomplishments have not been in science, and her daughter-republics in this hemisphere have followed in her footsteps. Until science is looked on more favorably and agriculture and other natural sciences are given the prestige they deserve—equal with other fields of endeavor—Colombia and her sister-republics can hardly expect to rear an independent-thinking and -acting corps of agricultural men who have faith in, and love and a deep-seated respect for, their work. Instead she will find her cities disproportionately filled, as at pres-

PICKING COFFEE ON THE COLLEGE'S DEMONSTRATION PLOT



VIEW FROM MAIN BUILDING, FACULTAD DE AGRONOMIA

THE BUILDING IN THE CENTER BACKGROUND IS A DORMITORY.

ent, with lawyers and physicians and her mountains and valleys crying for trained agriculturalists.

Students at the Facultad de Agronomía number about two hundred, and in recent years the enrollment has been gradually increasing. The undergraduate body is representative of all the agricultural regions of Colombia—tropical lowlands to temperate highlands—but the greater proportion of students come from the Caribbean coastal plain centering around Barranquilla and Cartagena. Tuition, ranging from 30 to 400 pesos annually, is logically graduated according to the taxable income of the father, thus permitting an educational opportunity for all classes. Further, certain worthy students lacking even the lowest financial wherewithal are enabled to attend school through funds supplied by their local home town. Colombia is a true melting pot of races, with all degrees of admixture between Spaniard, Indian, and Negro. Students in agriculture are a representative cross section of these many racial combinations.

As might be expected of a student body that is primarily studying agriculture because it can't be studying something else, study is listless and classes are dull, at least to a visiting professor accustomed to the alert and inquisitive U. S. student. While I was teaching at Medellín I failed to have a single student make a request for aid or for suggestions for additional readings in his chosen field. The average Colombian agricultural student will do only enough to just get by, and the diploma rather than what it represents seems to be the thing most desired. To some degree this lack of pep and vim may be laid to the absence of proper facilities for study and true campus life. Dormitories at the Facultad are inadequate, and so most students live in town, usually boarding and renting rooms in rather barren surroundings, where a study desk and quiet are unknown. At present, library study facilities are also

TYPICAL GROUP OF SECOND-YEAR AGRICULTURAL STUDENTS

inadequate, and most students resort to the Latin-American custom of pacing school corridors or patios, textbook in hand, all the while vigorously lip-reading the assignment. Such arrangements are not conducive to interest in any subject, and it may properly be said that the art of real study is foreign to most agricultural students in Colombia. Plans for a new building program have been recently made, and, when the funds have been allocated, the Facultad can look forward to an alleviation of present congested conditions.

Faculty-student relations are for the most part excellent. The visiting professor will find, however, at least one source of irritation —the *huelga estudiantil,* or "student strike." Into this weapon the students pour all the fire and energy that they refuse to give to the improvement of their education. In my opinion, the student strike is doing more to keep down high standards of teaching in Colombia—in secondary schools as well as in the universities—than any other one thing. The *huelga estudiantil* is apparently universal in Latin America; it is a sort of tradition. As used in Medellín, where I have seen strikes at several of the local colleges, it is inexcusable. One student strike at the Facultad de Agronomía lasted several weeks. Its immediate effect on my courses, as on everyone else's, was to necessitate the elimination of practically two weeks of important work, which could not be replaced in the school year. Laboratory preparations requiring several weeks' anticipation were thrown entirely off schedule. These losses were insignificant, however, compared with one of the principal causes of the strike—protest over the appointment and qualifications of a new professor whom the students did not like. Through their protest the students eventually caused the man's dismissal from teaching. If a strike of this sort were directed against poorly prepared professional men who should not be teaching, then such a weapon might be condoned. Unfortunately, this is not always the case, and strikes are sometimes called against competent teachers whose personalities do not appeal to the students, or against men who are giving too "tough" a course, that is,

the very teachers who are actually trying to lift educational standards and practices to a higher level. Thus it can come about that as fast as a Colombian institution improves its faculty a student strike can cancel the gain. For this reason the faculty unjustifiably has to "play ball" with the students, upon whom, in the final analysis, their tenure often depends. Until the National University, as well as other Colombian institutions, outlaws the *huelga estudiantil,* it will continue to lose its teachers, who are thus forced unwillingly into other fields by student whim.

THE school year, composed of two semesters, begins in February and ends in late November, with a between-semester vacation of several weeks in July. The curriculum at Medellín has been changed recently from a four-year to a five-year one. The principal reason for this extension undoubtedly lies in the fact that the school receives in its entering class such a mixed assortment of men—so far as their scholastic background is concerned—that it must fill in the necessary gaps caused by faulty secondary-school education. Thus, the first year is weighted heavily with mathematics and includes algebra, plane and solid geometry, arithmetic, trigonometry, physics, chemistry, freehand and industrial drawing, general biology, and introductory botany. Granted that mathematics is important, it is still overemphasized in a school that should stress agricultural subjects. This overemphasis is upheld with the argument that the degree offered (*ingeniero agrónomo*) is not simply one in general agriculture, but is more inclusive, with agricultural engineering entering prominently. The overemphasis of mathematics is an excellent example of a tendency in Latin-American institutions to offer all the subjects under the sun merely for institutional prestige and without carefully limiting the curriculum to the essentials

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or to courses that can be ably and creditably taught.

The curriculum throughout all five years at Medellín is a more or less inflexible one, and students get little freedom of choice so far as subjects are concerned. The courses given after the first year may also be of interest: In the second year the student takes differential and integral calculus, analytical geometry, analytical and organic chemistry, surveying, topography, mineralogy, geology, economic botany, plant propagation, and general zootechny; in the third year courses are agricultural machinery and repairs, agricultural economics, meteorology, agricultural chemistry, mycology, entomology, plant physiology, agronomy, and dairying; fourth-year courses include genetics, phytopathology, hydraulics, statistics, marketing, animal hygiene, horticulture, hybridization, pomology, principal farm and forage crops, temperate crops, and principles of irrigation and drainage; courses in the fifth and final year are tropical crops, control of disease, farm management, silviculture, cultivation of coffee, agricultural propaganda, production of Colombian agriculture, rural construction, agricultural technology, and two elective courses. Postgraduate studies are not offered, but may be given in the future, in which case specialization will be tentatively in fields such as phytopathology, entomology, genetics, taxonomic or physiological botany, chemistry, agronomy, agricultural engineering, agricultural economics, and zootechny. Although the list of subjects is fairly large, the caliber of courses offered is mediocre when compared with similar courses given in agricultural colleges of the United States.

One of the most pressing needs, not only in Colombian agricultural education but in general science education throughout all Latin America, is for good basic texts in Spanish. These should be written for tropical or warm-temperate Spanish America and should not be mere translations of texts designed for North American students. While I was teaching at Medellín, a local scientific supply dealer visited the school, offering, as teaching aids for Colombians, a full supply of typical North American charts—Audubon bird charts (including one on winter birds!), Riker mounts of life histories of North American plants and animals, and other such preserved materials from temperate regions. Needless to say, I laughed good-naturedly in his face and pointed out that we had initiated the construction of local charts using Colombian subjects whenever possible.

The visiting scientist is much surprised at first to learn that at Medellín a number of standard English texts—for instance, in biology, botany, geology, plant physiology, meteorology, soils, and genetics—are used in various courses. Think of a freshman attacking new and difficult subjects with texts written in a still-strange language! The lot of the embryo agricultural student is initially hard. There is a definite field for competent texts in the Spanish language. One big need is a Spanish-English technical dictionary, for many common scientific terms are as yet nonexistent in the Spanish language. There are, perhaps, a few English texts whose contents are sufficiently universal in application to serve Latin America in translated form, but the great majority would have to be either extensively revised or entirely rewritten. The factors in favor of translations from the English are the general high quality and modern presentation of the subject matter and the superior printing methods, for book quality is, as a rule, poor in Latin America—Mexico and Argentina excepted. Of course, preferably, Latin Americans should write their own texts, but this will be difficult until there is a larger corps of true Latin-American scientists. To be sure, a few Spanish agricultural texts are available, but many of these are quite out of date. Only a very few can be recommended for use in institutions of higher learning.

AMERICAN AND EURASIAN GLACIERS OF THE PAST: A PICTURE BASED ON EXISTING ONES

WILLIAM HERBERT HOBBS

Dr. Hobbs (Ph.D., Hopkins, 1888) is emeritus professor of geology and geography at the University of Michigan. He has been assistant geologist, U. S. Geological Survey, associate editor of the Journal of Geology, *director, University of Michigan Expeditions to Greenland (1926–29, '30), and vice-president of the International Glacier Commission.*

A CENTURY ago, when the theory was first promulgated that glaciers of continental dimensions once blanketed northern Europe and northern North America, the only glaciers of which much was known were those high up in the Swiss Alps. The existing continental glacier of the antarctic was not to become known for three-quarters of a century, though the margins of its vast expanse were being approached by three great national exploring expeditions while Agassiz was announcing his theory. The Greenland glacier was first to be explored during the last quarter of the nineteenth century, nearly a half century after Agassiz' discoveries.

In a measure, this explains why the geologists who surveyed and studied the deposits laid down by the Pleistocene glaciers did so with the picture of the little alpine glacier always in their minds. Their assumption that except for size and outline alpine and continental glaciers must be identical was quite without warrant. Even after the opportunity had arrived to learn something of the nature and the behavior of the earth's continental glaciers, glacialists have rather generally evinced but little interest in them.

The first studies of the periglacial area of an existing continental glacier were carried out by the University of Michigan Greenland Expeditions—five expeditions in the years 1926–32. It should not be a matter for much surprise that continental glaciers have now been found to be in their nature and in their behavior essentially different from the diminutive alpine ones, which had so long served as models in interpreting the great glaciers of the past.

The more important of these differences relate to—

1. Their form or model.
2. The slope of their underlying bed.
3. The generally imperceptible slope of their upper surface.
4. Their general lack of internal movement.
5. Their overlying centrifugal wind system.
6. The place and manner of their nourishment.
7. The nature of the bed they override.
8. The place and manner of their deglaciation.
9. The agents and the nature of the deposits they lay down.

The model of a continental glacier is a very flat dome, or shield, though it may be likened to the cover of a hunting-case watch or a cake of very thin batter, the surfaces of which are, with the exception of the relatively narrow intramarginal zone, flat, though everywhere with a hardly perceptible outward slope. Unlike the alpine glacier, there is beneath the continental glacier no definite forward slope of the bed that could bring about a gravitational flowlike internal movement in the glacier after it has attained its maximum size. The radial profile through a continental glacier, such as that of Greenland, could be represented in correct proportions (with vertical and horizontal scales the same) by a thin horizontal pencil line about six inches in length.

The upper surface of an alpine glacier slopes steeply forward by an amount approximately that of its bed, but the surface of a continental glacier, except within a relatively narrow intramarginal zone, is so flat that its gradient is imperceptible to the unaided eye—one or two degrees only of arc—though the theodolite reveals that this slope is always outward from a central divide. Internal ex-

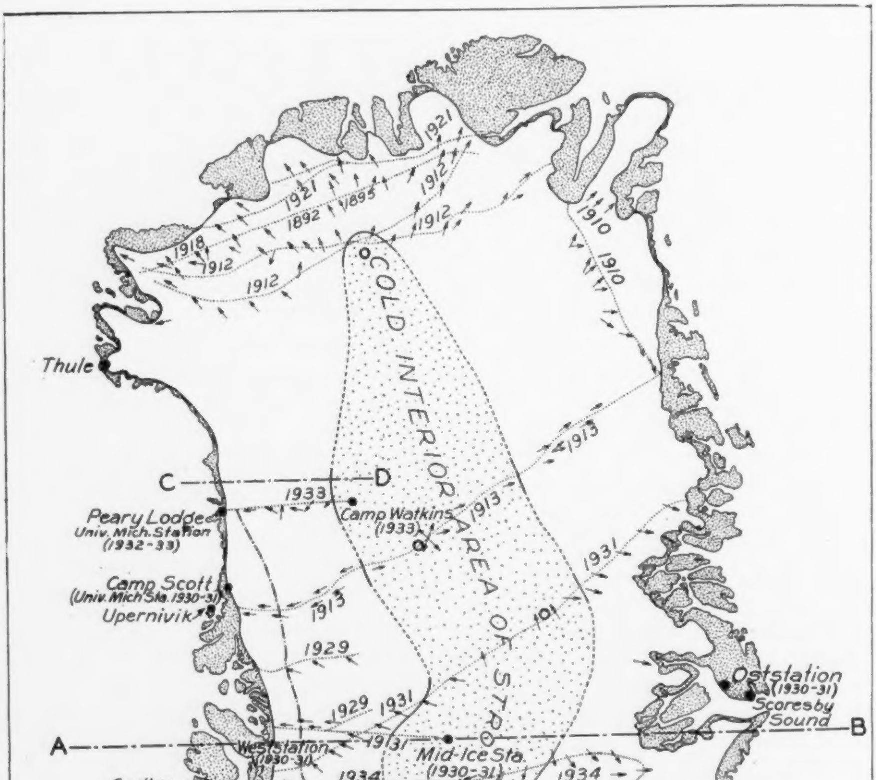


FIG. 1. THE GREENLAND CONTINENTAL GLACIER

THE SMALL MAP (*lower right*) SHOWS THE CENTRIFUGAL WIND SYSTEM (GLACIAL ANTICYCLONE) THAT OVERLIES AND SURROUNDS THE GLACIER. THE DOTTED LINES ON THE LARGE MAP ARE THE TRACKS OF EXPLORERS, AND THE ARROWS ON THE SAME MAP INDICATE THE DIRECTIONS OF THE STRONG WINDS THEY ENCOUNTERED.

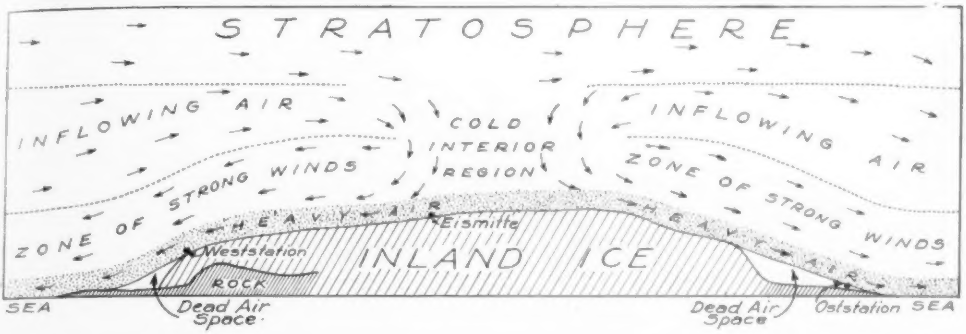


FIG. 2. TRANSVERSE PROFILE OF THE GREENLAND CONTINENTAL GLACIER

SECTION ALONG THE LINE *AB* OF FIGURE 1, BUT WITH THE VERTICAL SCALE ABOUT 300 TIMES THE HORIZONTAL.

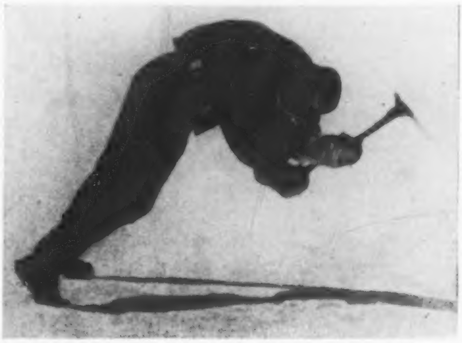
trusion-flow after the glacier has attained its full spread must therefore be restricted to the steeper intramarginal zone, within most of which such a flow is betrayed by the cracks (crevasses) on the glacier surface. The surface of the glacier is held to its model, despite its intramarginal extrusion-flow, by the snow that is drifted outward over its surface by the winds of the glacial anticyclone; and its surface layers are, except within the outer zone of summer surface melting, always of snow, never of ice.

A strong centrifugal wind system, the glacial anticyclone, is always present above a continental glacier. These winds extend outward over the periglacial region for at least a hundred miles beyond the glacier margin, and they may extend hundreds of miles beyond it (Figs. 1 and 2). This powerful system, with wind velocities that at times surpass one hundred and fifty miles per hour (Fig. 3), is apparently brought about as a consequence of the refrigeration—abstraction of heat from the air layers close to the glacier surface—and of the consistently outward-directed surface gradient of the glacier.

Though the alpine glacier is nourished by the snow precipitated from ascending currents of moisture-laden air that move in over the glacier from its front, such a movement is not possible for the continental glacier, because of the outblowing winds of the glacial anticyclone. Its nourishment is supplied by the ice needles of the cirri contained in descending air currents from the substratosphere within the downdraft of the central core of the anticyclone (Figs. 1 and 2). These ice needles, adiabatically vaporized during the central downdraft, are, as fine "frost snow," precipitated just above the glacier surface and are swept outward toward the borders by the winds of the glacial anticyclone. At stations set up in this downdraft over Greenland, such snows and driftings occurred on more than half the days that the stations were occupied. The nourishment of a continental glacier is thus supplied by descending air currents, both above the cold central area and, by drifting, over all flanks of the glacier as well.

This central area of downdraft of the glacial anticyclone is very sharply delimited on all sides. Explorers, as they enter it, are struck by the much lower temperature of the air, and by the greater daily range of air temperature, though this border is marked by no topographic break (Fig. 1).

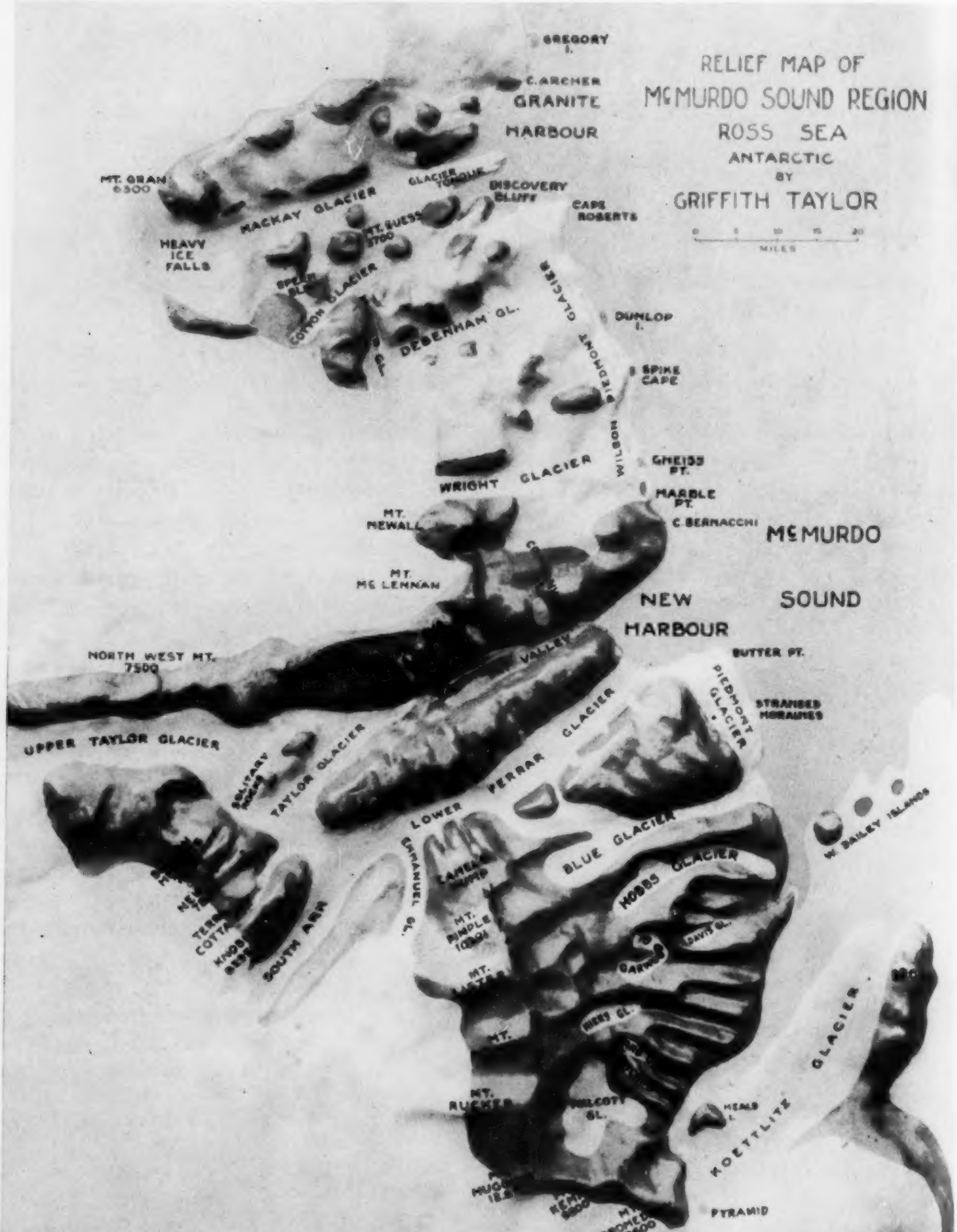
The alpine glacier is perched high upon



After Mawson

FIG. 3. LEANING ON THE WIND

FACING THE ANTICYCLONE, THE MAN IS CHOPPING ICE.



After Griffith Taylor

FIG. 4. THE BORDER OF THE ANTARCTIC CONTINENTAL GLACIER

THE ROUNDED SUMMITS OF GLACIATED MOUNTAINS HAVE BEEN EXPOSED BY DOWNMELTING OF THE SURFACE OF THE CONTINENTAL GLACIER. THE MEASURE OF DOWNMELTING HAS HERE BEEN IN EXCESS OF 2,000 FEET.

a bed too steep to hold weathered mantle rock. It has therefore had throughout a bed of indurated rock, which by reason of its forward downslope the glacier has been competent to pry off in blocks. With these as tools the bedrock is ground down and glaciated (planed off, polished, grooved, and striated). In the process the blocks are themselves ground away on the faceted surfaces, so that the deposits of the alpine glacier are powdered rock in which are imbedded faceted boulders and pebbles.

The continental glacier lacks a definitely sloping bed and so advances over weathered mantle rock, or regolith, with its included residual rounded blocks of the harder rocks, such as granite, gneiss, and the crystalline rocks rather generally. This regolith material it ploughs or scrapes up, together with any trees, turf, or soil that had covered it. The rounded residual boulders are for the continental glacier the ready-made tools with which it can glaciate the bedrock. These residual blocks of the crystalline rocks are sometimes of gigantic dimensions, and after transportation by the glacier they are left, except as drilled by flying sand, with their outer surfaces but little changed. They apparently travel close to the front of the glacier, and the larger ones are not overturned during transit, since they are found to be glaciated (planed off and polished) on a single side only, the under one. They are found, as a rule, not on the land the glacier has overridden, but outside on the periglacial areas, over which they have moved in melt-water floods while encased in detached islets of glacial ice. Quite by contrast with the faceted and glaciated surfaces of the boulders of alpine glaciers, they may be described as saxums (from *saxum glacie transportatum*).

The place and the manner of deglaciation during the receding hemicycle of glaciation are as different as possible for alpine and continental glaciers. The alpine glacier is nourished by snow deposited near its head, far up near the crest of a lofty mountain range, but it is melted far below near its foot, where much higher air temperatures and, far more important, where warm summer rains prevail. Melting of the glacier is there-

FIG. 5. A FORMER GLACIER

A LOBE OF THE NEXT-TO-THE-LATEST GLACIER IN NORTH AMERICA, HERE DESIGNATED THE MANKATO LOBE OF LATE ILLINOIAN AGE. THIS IS SURROUNDED BY AN OUTWASH PLAIN WHICH ONCE HAD A GREAT NUMBER OF GRANITE SAXUMS SCATTERED OVER IT.

fore concentrated at the glacier foot, and as it proceeds the glacier front retires up the valley. The sediment-filled melt water that issues from the glacier front is on so steep a slope that it has a swift current, and its burden of rock debris is largely carried down to the distant lowland, where it is widely dissipated. Glaciofluvial deposits thus have small importance in the case of alpine glaciers.

With continental glaciers it is quite different. Study of the Greenland and the antarctic glaciers has shown clearly that in the deglaciation that characterizes the present era, when all glaciers are in the receding hemicycle of glaciation, these glaciers are melting on

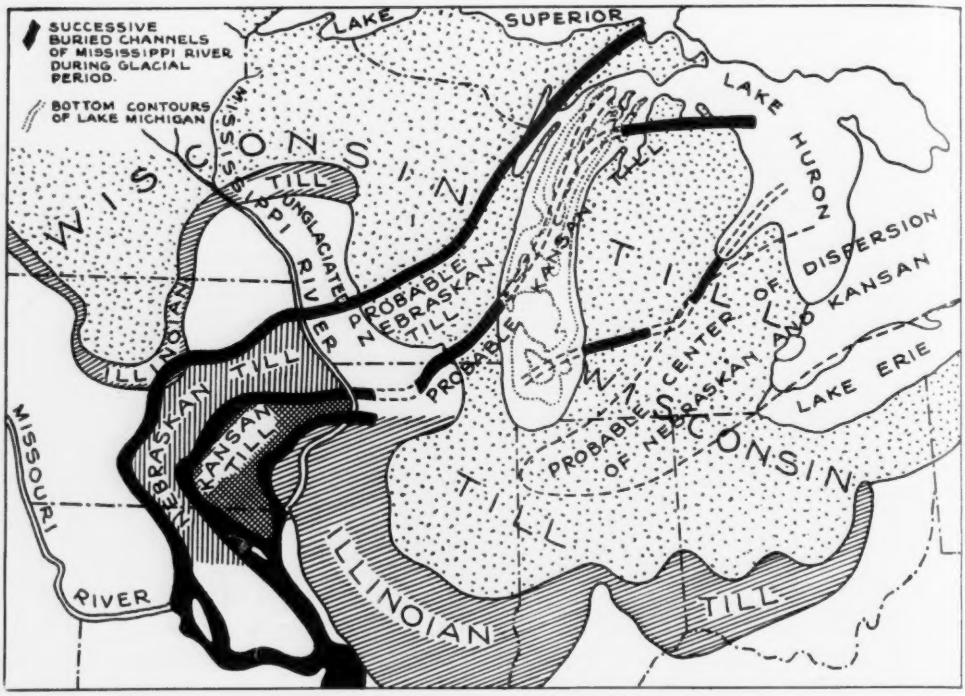


FIG. 6. GLACIAL MAP OF THE UPPER MISSISSIPPI VALLEY REGION

SOMEWHAT EXAGGERATED AS TO WIDTH, THIS MAP SHOWS THE MELT-WATER RIVER CHANNELS MARGINAL TO THE NORTHERN FLANKS OF THE THREE EARLIER GLACIERS—THE NEBRASKAN, KANSAN, AND EARLY ILLINOIAN. TO THE NORTHWEST OF THEM IN IOWA AND MINNESOTA THE MAP SHOWS THE FRONTAL LOBES OF TWO LATER GLACIERS, THE LATE ILLINOIAN AND THE LATE WISCONSIN, BUT FROM THEIR FRONTS THE LAND SLOPED AWAY, AND IN PLACE OF MARGINAL RIVERS THERE WERE OUTWASH PLAINS, WHICH HAVE NOT BEEN REPRESENTED HERE.

their upper surfaces, not back from their fronts as do alpine ones (Fig. 4).

Melting of glaciers takes place only in the summer seasons. By late midsummer the glacier surface, within an outer frontal zone (in West Greenland some tens of miles in width), is always covered with melt-water rivers and lakes. These superglacial rivers flow outward downslope to descend in the intramarginal crevasses, continue through subglacial tunnels, and issue from beneath the glacier front under hydrostatic pressure and with a strong current. During the day the melt water floods the land surface throughout, over the frontal periglacial area, but toward evening the outflow is reduced to streams within channels of braided pattern. The glaciofluvial deposits thus build up an outwash plain whose inner margin is the glacier front and whose outer border is of dendritic pattern. From this front small icebergs become detached through undermining, and these float off loaded with boulders and finer englacial rock debris of all sizes. These bergs either float out to the sea in the outwash channels, or during the floodings of the plain they are left stranded upon its surface, where the ice melts and leaves the boulders behind.

At the end of the summer season the glacier melt water ceases to issue, and surface layers of the outwash plain dry out. The outblowing winds of the glacial anticyclone, throughout the summer powerless to lift even the finest silt, now take over as the transporting agent, and under their action the plain becomes a deflation area. Not only silt, but sand and even pebbles, are lifted and carried in the air across the outwash plain—the pebbles by saltation, the coarser sand a few feet only above the ground, the finer sand and silt horizontally at higher levels. Left behind as

FIG. 7. THE LATEST GLACIERS OF NORTH AMERICA AND EURASIA

THESE GLACIERS WERE SURROUNDED BY APRONS OF GLACIOFLUVIAL AND GLACIOEOLIAN DEPOSITS. THE CORRESPONDENCE IN AGE OF THESE APRONS OF OUTWASH AND LOESS WITH THE GLACIERS THEY ENCLOSE IS ATTESTED BY THEIR VERTEBRATE FOSSILS. THE EURASIAN AREA OF THIS MAP IS BASED ON A RECENT U.S.S.R. ATLAS.

an armor to the plain is a coarse "pebble band" made up of materials too heavy to be lifted, and this band of armor soon halts the deflation process. While in the air the coarser sand bombards the stranded boulders and transforms them into ventifacts; if these are intersected by dikes or veins, into those of ribbed types, deeply recessed.

The floods of the next succeeding summer redistribute the surface pebble band to the plain and whirl the stranded boulders about so as to expose a new face to the winter sandblast, thus ultimately giving the boulders an ellipsoidal form. Repeated movements through successive summers keeps them always on the surface of the plain as it rises. With the retirement of the glacier front the latest pebble band alone survives as a permanent armored surface to the plain, together with the scattered ventifact boulders.

The silt, which has traveled beyond the outwash, has been deposited and held down by the tundra vegetation, for plant growth is inhibited on the outwash itself by the floodings throughout the growing season. Thus, during the deglaciation of a continental glacier, wherever the general land surface has a slope *away* from the ice front, an outwash plain will have formed as an apron to the glacier, and outside it on the periglacial area an apron of glacioeolian deposit (silt or loess), which is thickest next the outwash and which thins out gradually throughout a distance of one hundred miles or more.

The deposits thus laid down over the periglacial region during the receding hemicycle of continental glaciers may be no less important than the glacial ones. Some thousands of cubic miles of melt water issuing on a single glacier flank, and all of it highly charged with suspended sediment, cannot be left unconsidered. It is probably because melt-water deposits are so unimportant in the case of alpine glaciers, and because these have been the models in interpreting the Pleistocene deposits, that the glaciofluvial deposits have been generally overlooked by students of glaciers.

It must be clear that outwash aprons will be formed only where the general slope of the land is *away* from the glacier front. If *toward* it, their place will be taken by a great marginal melt-water river. Such marginal melt-water rivers flowed mainly in the summer seasons, but they acquired proportions large as measured even by our present Mississippi River. For the three earlier of the Pleistocene glaciers of North America, such melt-water rivers are revealed by channels cut deep in bedrock, but now filled to the brim by outwash deposits of the later glaciers (Figs. 5 and 6).

The lobes of the later glaciers of the Upper Mississippi Valley in Iowa and Minnesota, the Late Illinoian and the Late Wisconsin, spread out southward over a flat divide from which the land sloped away on all sides. Both were in consequence surrounded by inner aprons of outwash with loess aprons outside.

Heretofore regarded as true glacial deposits (Iowan), this "two-story" outwash plain of glaciofluvial deposits has a lower portion (Illinoian) that is largely buried beneath the upper (Wisconsin). Both outwash plains have a surface pebble band, and the upper plain has scattered ventifact boulders, which have been drilled by the sandblasts. By contrast, the lower plain was once strewn with gigantic saxums glaciated on their under surfaces and projecting through the surface of the overlying deposits. They have now disappeared because they have been quarried for local building stone.

Glaciofluvial and glacioeolian deposits of the Pleistocene of North America thus offer a close parallel with those observed in Greenland off the border of its still-existing continental glacier. The North American and Eurasian glaciers of the latest (Wisconsin) stage (Fig. 7) have been rather generally surrounded by inner aprons of outwash gravels and outer ones of loess.

THE TOX LAB

JOHN O. HUTCHENS

A native of Noblesville, Indiana, Dr. Hutchens received his Ph.D. from Johns Hopkins University in 1939. He was a National Research fellow in zoology at Harvard during 1939–40 and a Johnston scholar at Johns Hopkins in 1940–41. Dr. Hutchens then became professor of physiology at Chicago and is now director of the Toxicity Laboratory there.

IN THE rear of a big parking lot on the University of Chicago campus sits a group of low tile and brick buildings surrounded by a high wooden fence topped by several strands of forbidding barbed wire. Close by stands the smokestack of the old University powerhouse, and into its base a bank of huge blowers pours the air from a maze of 18-inch ducts that converge from the laboratories inside the fence. Throughout the war these outward aspects were all the outside world was allowed to know about the University of Chicago Toxicity Laboratory.

Scientists of the United States and allied nations engaged in chemical warfare research knew, however, that to the Toxicity Laboratory were funneled all the new candidate chemical warfare agents synthesized by O.S.R.D. chemists throughout the nation. There they were tested to find out whether they were lethal if inhaled, would blister the skin, or do any of the possible kinds of damage that would allow them to supplement or replace the mustard gas, phosgene, and other war gases of World War I, including the highly touted Lewisite.

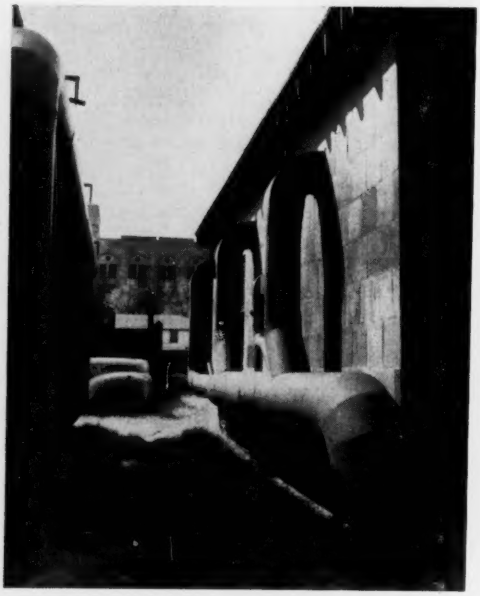
The administrative history of the Toxicity Laboratory goes back to April 1941 when a contract was established between the University of Chicago and the National Defense Research Committee of the Council of National Defense. Dr. E. M. K. Geiling, professor and chairman of the Department of Pharmacology, was named official investigator, and Dr. Franklin C. McLean, professor of pathological physiology, was appointed as the first director in charge of research. In two years, under the direction of Drs. Geiling and McLean, the Toxicity Laboratory grew to its present size from the single original building comprising one experimental laboratory and animal quarters, with a staff of half a dozen scientists working.

Today the staff numbers half a hundred scientists and technicians working in seven large laboratories and an administration building inside the big fence and in laboratories in the Departments of Physiology, Pharmacology, and Pathology. The large animal quarters house all sorts of animals, from monkeys and mice down to the cockroaches and silkworms used in studies of insecticides and basic problems in biology.

In 1943 Dr. McLean resigned as director to accept a commission (as Lt. Colonel, Medical Corps) for service with the Medical Division, Office of the Chief, Chemical Warfare Service. Dr. R. Keith Cannan, professor of chemistry, New York University, became the second director of the laboratory and served until March 1945. From May 1942 until this time Dr. William Bloom, professor of anatomy at the University, was in charge of testing the skin-blistering properties of compounds and the effectiveness of protective ointments.

In March 1945 the O.S.R.D. was being dissolved, and support of U.C.T.L. was undertaken by the Chemical Warfare Service. Dr. William L. Doyle, now associate professor of anatomy, became director and served until March 1946, when I succeeded him. Throughout the period of its existence, the directors and staff have had the advantage of a large Advisory Committee with representatives from many departments in the Biological and Physical Science Divisions of the University of Chicago.

To those who worked there through the hectic days of 1941–43 the growth in size of the physical plant of the Toxicity Laboratory was an easy accomplishment as compared with assembling a staff, defining research



VENTILATION SYSTEM, TOX LAB

problems, and gaining the experience necessary to interpret experimental results in terms of the needs of the armed services. In common with other scientists throughout the country who were requested to engage in defense research, those at Toxicity Laboratory asked one another, "What do they want to know?" Not only were we unacquainted with the strategic and tactical problems of the military but we had not the slightest knowledge, at the outset, of the state of development to which the pitifully few scientists in the laboratories of the Chemical Warfare Service at Edgewood Arsenal had been able to bring their problems. A second problem in staffing a wartime laboratory was created by the shortage of trained scientists. Men had to be obtained, some virtually kidnapped from prewar jobs, to do whatever was to be done.

In the meantime, something had to be done about the tens, then hundreds, and finally thousands, of chemical compounds that were arriving by mail, express, and personal messenger. Each, it was the fond hope of the chemist who had synthesized it, was the new superchemical warfare agent. They had to be tested. What would they do? As soon as the staff undertook such tests, one thing became abundantly clear—the sum total of knowledge in biology, medicine, and the physical sciences would have to be used. So, of their own volition, or responding to urgent pleas for help, pharmacologists, physiologists, biochemists, pathologists, chemists, physicists, mathematicians, ophthalmologists, and dermatologists joined the group.

Compounds were tested for every manner of effect they might have on the animal body—first, of course, for toxicity when inhaled. But this was not enough. Did it cloud the eye or blister the skin as did mustard gas? Did it make a man cry, or sneeze, or his skin itch? And then, if it did any of these things, why? And what could be done about it? How good were the gas masks, the antigas ointments, protective clothing? Along with other O.S.R.D. laboratories and the rapidly growing Army and Navy installations, Toxicity Laboratory scientists tackled all these problems.

The answers of general interest have either been, or will be, published in open scientific journals. Others of more direct military importance still reside in secret or confidential reports. Many of the contributions to medicine that grew out of this work have been outlined by Bodansky (*Science,* 102, p. 517).

TODAY, Toxicity Laboratory scientists are having an opportunity to pursue some of the ideas that occurred to them as they investigated the several thousand chemicals submitted to the laboratory during the war. Many compounds useless as chemical warfare agents were put aside in the knowledge that they acted in strange ways on one or more of the organs or tissues of the animal body. It is a curious fact that scientists learn most about the normal function of these tissues and organs by observing the effects of departure from normality. The importance of an endocrine gland is discovered when it is removed or if for some reason it becomes hyper- or hypofunctional. The metabolic role of a particular enzyme is disclosed if its activities are inhibited. Among the compounds studied, many appeared that would cause a chemical removal of an enzyme, a cell type, or an organ from participation in the body economy. Others stimulated organs to excess activity. Here

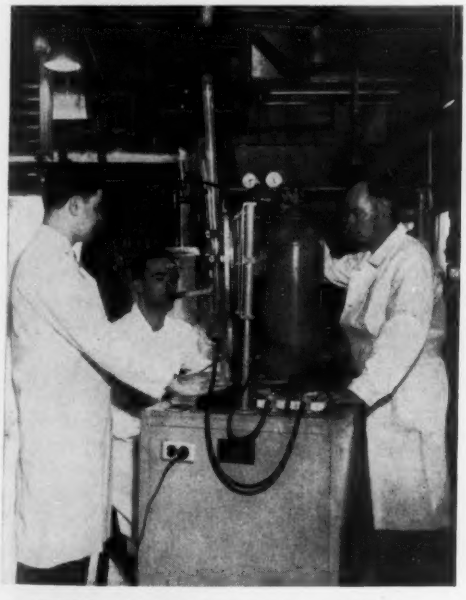
were chemical tools to be exploited in physiology, pharmacology, biochemistry, and clinical medicine!

Probably the best known of the agents discovered in chemical warfare research are the nitrogen mustards. This story began at the Toxicity Laboratory in 1942 when Dr. C. C. Lushbaugh, pathologist, first noted that mice gassed with nitrogen mustards had many fewer white blood cells than normal and that the bone marrow and lymph nodes of the animals no longer were forming blood cells.

This finding stimulated various groups, including Drs. Leon Jacobsen and Charles Spurr, in the Department of Medicine at the University of Chicago, to test the effectiveness of the nitrogen mustards against certain diseases such as leukemia, lymphosarcoma, and Hodgkin's disease, which are characterized by the presence of abnormally large numbers of white blood cells. The compounds were found to have effects like those of X-rays.

What is not so well known is that there are many "nitrogen mustards" yet to be tested, first on laboratory animals and, if the findings warrant, on patients suffering from those neoplastic diseases. In 1942, when intelligence reports indicated that the Germans were experimenting with an agent called "Stickstoff Lost" (*Stickstoff,* "nitrogen"; *Lost,* the German name for the mustard gas of World War I), an intensive search for a nitrogen-containing compound that had the properties of mustard gas was instituted. O.S.R.D. chemists synthesized a large number of such compounds, and these were tested at the Toxicity Laboratory. Those most toxic, most vesicant, and most damaging to the eyes were studied in great detail.

STUDYING OXYGEN CONSUMPTION



TESTING PENICILLIN AEROSOL

All were amines (organic ammonialike compounds) bearing at least one β-chloroethyl group. One of these, bis (β-chloroethyl) isopropyl amine, received the most attention in clinical studies. Nothing was more natural than to employ the compound about which most was known from animal studies. One must bear in mind, however, that the object of the original experiments was to find a very toxic compound. For therapeutic purposes, this must be used in small doses. In recent months it has been possible to restudy some of the nitrogen mustards set aside as relatively nontoxic in 1942–43. It has been found that some of these affect white cells while having a lesser toxicity; that is, they have a better potential "therapeutic index." More nitrogen mustards can be synthesized for testing.

The nitrogen mustards are only one of the

INVESTIGATING PHARMACOLOGICAL ACTION OF A DRUG ON A RABBIT

groups of compounds of interest in cancer research. Among the several thousand chemicals tested during the war are many related to types that have shown some indication of activity against cancer. From this store and from the accession lists of the Chemical-Biological Coordination Center of the National Research Council, new candidate agents are being drawn in the search for one more effective against cancer. Through the Chemical-Biological Coordination Center, compounds stored at the Toxicity Laboratory are made available to other scientists interested in using them for cancer research, to produce genetic mutations, as enzyme inhibitors, and for many other purposes.

Potent poisons discovered during the war also have potential usefulness in the control of insects and rodents. The well-known 1080 (sodium fluoroacetate) was first studied, in the form of the methyl ester, as a chemical warfare agent.

Among the compounds at the Toxicity Laboratory are many having a high toxicity for rats, mice, insects, and other pests. They cannot be employed as insecticides or rodenticides, however, until intensive investigations have established several other facts about them. The requirements for an ideal insecticide or rodenticide are many. It should have little toxicity for desirable animals, its symptoms of poisoning should be distinctive, and there should be a method of treatment in case of accidental poisoning. These and many other properties of the compound must be studied before field trials are undertaken. Since 1945 a large group of workers in the Toxicity Laboratory has concentrated on the study of insecticides and rodenticides, investigating both new compounds and extending observations on those already in use.

All the available rodenticides fail in one or more ways to match the ideal. For example, rodents exposed to a nonlethal dose of ANTU

(α-naphthyl thiourea) become resistant, so that further dosing is ineffective. It is potent against the Norway rat, but not so effective against the Alexandrian rat. 1080 is poorly accepted by some rodents, and it is more toxic for other animals, such as dogs and cats, than for rodents. No effective specific method for treating ANTU or 1080 poisoning has yet been discovered. Nevertheless, both compounds have been extremely useful in rodent control, and probably will continue to be used until better agents are found.

Groups under Drs. K. P. DuBois, G. H. Mangun, J. M. Coon, and J. O. Hutchens are studying ANTU, 1080, the German rodenticide "Castrix," and a variety of other agents, primarily phosphorus compounds in which many phosphorus atoms are linked together by oxygen atoms (esters of polyphosphoric acids). Scientists interested in the results of these studies are rapidly informed through the medium of a bimonthly progress report. When brought to a satisfactory stage of completion, the results are published in scientific journals.

Of the many compounds studied, the polyphosphoric acid esters are among the most interesting. During the war di-isopropyl fluorophosphate (DFP), which contains only one phosphorus atom, was discarded as a war gas, but its ability to inhibit an enzyme (cholinesterase) important in nerve conduction suggested it as a possible substitute for the drug eserine. At the University of Pennsylvania, Drs. Irving H. Leopold and Julius H. Comroe found it useful in reducing the abnormally high tension of the eyeball occurring in glaucoma. These workers and Dr. A. McGehee Harvey and others at Johns Hopkins tested DFP in myasthenia gravis, a wasting disease of the muscles that can be treated by eserine. In some respects it was more effective than eserine, but there were undesirable side effects.

TESTING THE EFFECTS OF MUSTARDS ON ANTIBODY DEVELOPMENT

When hexaethyl tetraphosphate (a four-phosphorus compound) was introduced as an insecticide, Drs. Mangun and DuBois suspected that, like DFP, it acted by inhibiting cholinesterase. This proved to be true, so a long series of polyphosphate esters were investigated. One in particular, tetraethyl pyrophosphate (TEPP), is a more potent anticholinesterase than any other tested. By altering the chemical groups surrounding the phosphorus atoms, the enzyme-inhibiting action of the polyphosphates and other of their chemical properties can be altered almost at will. The potential practical usefulness of these compounds is in itself enough to stimulate investigation. To the physiologists, pharmacologists, and biochemists pursuing the study, the additional light that may be shed on mechanisms whereby nerves conduct their impulses is an even more intriguing problem.

Experimental techniques developed during the study of inhalation of toxic compounds also have been found important for peacetime use. Among the chemicals studied at U.C.T.L., many consisted of such large complex molecules that they were nonvolatile and had to be dispersed in the air as finely divided mists or dusts (aerosols).

Methods were developed for measuring the size of these aerosol particles and for studying their behavior in the respiratory tract. It was of course known that a large dust particle, if breathed through the nose, would be removed there and would not reach the lung, and that fine particles such as those in tobacco smoke could be breathed into the lung and then many of them exhaled. The techniques developed by Dr. H. D. Landahl and his associates permit quantitative measurement of the percent of particles of any given size and density that would be retained in the nose or lung at a given rate of breathing. Inhaling aerosols of corn oil, sodium bicarbonate, calcium phosphate, and glycerine water mixtures, they learned the importance of particle diameter, density, velocity of the particle as determined by breathing rate, and mean time spent by the particle in the lung.

These techniques have already proven useful in studies of the effectiveness of aerosols of the antibiotics, penicillin and streptomycin, in treatment of respiratory infections. It has been possible to measure the actual dose of antibiotic retained in the body and thus compare it with the amount that would have to be injected to obtain a similar therapeutic effect. In collaboration with Dr. Robert Bloch, professor of medicine, and Dr. William Adams, associate professor of surgery, Drs. Erwin Levin and G. H. Mangun, of U.C.T.L., have studied the effect of penicillin and streptomycin aerosols on a variety of respiratory infections.

The information gathered in these and Landahl's studies promises to be of use in even more important ways. Such diseases as silicosis and asbestosis result from retention of fine dust particles in the lung. Knowledge of nasal filtration and the retention in the lung of particles of known size should permit more accurate prediction of the hazard created by industrial dusts and fogs.

THESE are but brief sketches of some of the varied research interests of Toxicity Laboratory. With a staff composed of members of the Departments of Physiology, Pharmacology, Biochemistry, Mathematical Biophysics, Pathology, and Medicine, interests are bound to be broad. To the scientists of each of these fields every new problem offers a special challenge and is susceptible to a different mode of attack—each new finding has a special significance. More than six years of association, struggling with common problems, has taught these scientists to depend on each other for stimulation, advice, and assistance.

This mutual respect and dependence makes itself felt in those departments of the University that furnish staff members in the Toxicity Laboratory. Students reap the benefit of the broadened experiences of their instructors. Some graduate students carry out in the Toxicity Laboratory the research on which their Ph.D. theses are based, working under a staff member from their department of specialization. Thus, these graduate students, the research men of tomorrow, can draw on the store of new techniques and new chemical tools as they are being discovered.

THE LIFE OF THE WATER FILM*

LORUS J. MILNE AND MARGERY J. MILNE

Reprinted by permission from Natural History *(June 1947), the following article won honorable mention in the 1947 AAAS-George Westinghouse Science Writing Awards. The Milnes are free-lance writers whose fascinating natural-history stories and photographs have appeared in such magazines as the* Atlantic Monthly, Fauna, Nature Magazine, *and* Popular Photography. *Dr. Milne is associate professor of zoology, University of Vermont; Mrs. Milne is assistant professor of botany at the same University.*

ONE of the first facts learned by every child is that "water is wet." In more mature years this wetness is so taken for granted that any exceptions to the rule arouse great interest. Thus the sewing needle that can be lowered gently onto the surface of a tumblerful of water, there to float completely dry, is a startling discovery. Yet to a large number of different animals and plants, this problem of wetness and dryness is a matter of life and death. Many of them find the "dry" surface of water to be a place to live, albeit precariously. Suspended between the air above and the depths below, they inhabit the surface film of ponds, streams, lakes, and even oceans. Theirs is an almost two-dimensional realm, a special niche in nature for use of which certain requirements must be met.

When a substance attracts water molecules more strongly than water molecules attract each other, the water wets the surface. The liquid creeps along, invading every crevice, clinging tightly to each irregularity. But some materials, such as waxes and oils, attract water molecules so little that the water draws away, pulling back into itself and leaving the surfaces dry. Aquatic birds take advantage of this principle by regularly adding oil to their outer plumage, thereby keeping their feathers from becoming water-soaked. The many creatures that walk on water do so by means of well-waxed, hair-booted feet which the water cannot wet.

Best known of all the animals that walk dry-shod on ponds and streams are the water striders—bugs with four long legs stretching out to the sides and a shorter pair held under the head. Their slender feet are covered with greasy hairs which the water fails to penetrate. Each foot presses the water surface and makes a dimple in it, but the water does not run around and let the foot fall through the surface film as it would if the waxy hairs were absent. Instead the insect's weight is supported partly by the buoyant force of the water displaced from the dimples and partly by the surface tension, which tends to erase the depressions and bring all the water film to the same level. The strider uses chiefly its hind- and foremost legs to hold its body well above the smooth and slippery surface of the pond, while working the middle pair as oars to scull itself along. Mirrored in the water film below the bug is its image—a reflected "double" seldom seen except by small creatures close to the water surface. Below the strider, on the bottom, are dark shadows cast not only by the insect, but also by the dimples in the surface film where its feet press downward. Sometimes, on sunny days, these shadows on a sandy bottom are more conspicuous than the insects themselves. They drift along and follow every movement of the rowing striders on the film above.

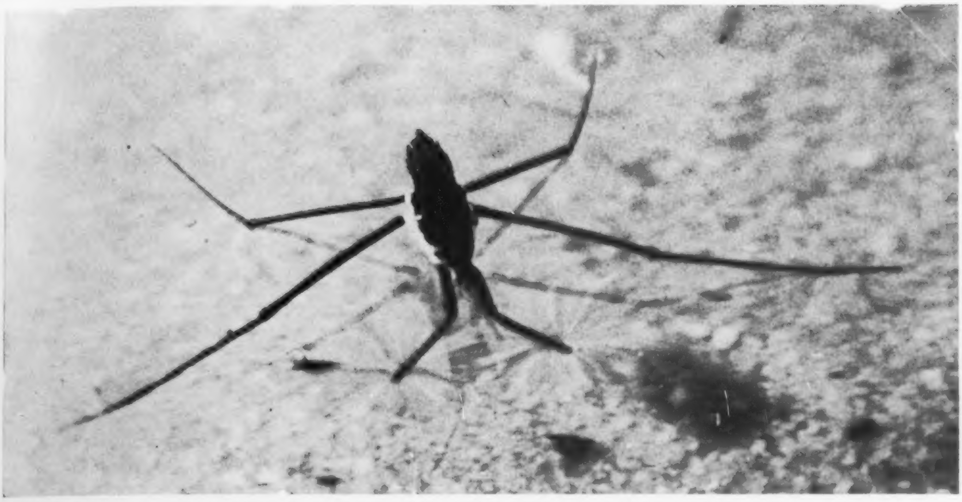
A considerable *length* of surface must be called upon to support an insect as heavy as a full-grown water strider. If its hair-booted feet pressed on the film at only six small points, the bug would penetrate into the water and sink at once. But the strider's legs are

* Both text and illustrations are from *A Multitude of Living Things* (New York: Dodd, Mead, 1947. Chap. 11), copyright by Lorus J. Milne and Margery J. Milne.

spread so widely that its feet make elongated dimples or furrows in the water film. So secure is the insect on a quiet pond or stream that it can shift its weight freely among its feet. Most spectacular are the demonstrations of this when a strider cleans itself.

A QUIET POND

HERE THE NATURALIST HAS AN EXCELLENT OPPORTUNITY TO STUDY THE LIFE OF THE WATER FILM.



A WATER STRIDER ON THE SURFACE OF A POND

THE INSECT ROWS DRY-SHOD ON THE WATER. DEPRESSIONS IN THE FILM APPEAR UNDER THE BUG'S FEET.

Drawing its rowing legs far back, it stands with its head almost in the water, while its hind legs are raised well above the surface and rubbed one against the other much as the housefly does. Then the insect rests on forefeet and one hind foot, with the rowing leg on that side as an outrigger, while the middle and rear feet of the opposite side are raised into the air and rubbed free of clinging particles by a similar fiddling movement. To accomplish this contortion, the bug practically lies down on its side. The water film stands the strain, but the shadows cast on the bottom shift and spread as the pressures on the fewer surface furrows are increased. Finally, the strider stands on rowing feet and rear pontoons while its body and forelegs are raised high above the water. The insect washes itself much as a kitten does, transferring dust particles from feelers, beak, and body to the forefeet, then rubbing these together until they are satisfactorily clean. The bug seems to give great care to every detail, and if uninterrupted, such a complete toilet operation may take ten minutes.

Other animals that spread their weight on outstretched feet can rest with safety on the water film. Small gnats and midges flit from place to place on ponds, alighting with equal equanimity on film or foliage. Even large crane flies settle with surprising grace upon the water surface, and rise again with their long legs trailing behind. Each foot combines the advantages of waxed hair covering and slender length which can distribute the insect's weight along the surface film. Sometimes the water striders catch small flies that fail to take flight in time. Occasionally, too, a crane fly dies of unknown causes while resting on the water surface. Without muscle action to hold its body well above the water, the insect sags into the pond and sinks in or barely stays afloat. Water striders gather around it to salvage such nourishing juices as remain. It is but one of the many types of food the striders seek as they push their way along the transparent but rubbery surface film. Some of their sustenance floats up to them from below; each dead fish attracts a crowd of striders. But most of the food of these insects falls into the water from the air above. Ants tumble from leaves overhanging the water. Beetles close their wings and drop or blunder into ponds and streams, where the water wets them and renders them helpless prey to the predaceous bugs. The striders investigate every particle, often making great leaps over the water to reach some newly fallen object. Small particles such as drowning ants are picked out

TWO STRIDERS AND THE CAST SKIN OF A *DOLOMEDES* SPIDER

ONE WATER STRIDER CARRIES AN ANT IMPALED ON ITS BEAK WHILE REMOVING THE NOURISHING JUICES.

of the surface film and held on a slender black beak while the life juices are drained away. Often a strider is seen carrying with it a gnat or other carcass as it glides along the water surface. Occasionally other striders chase the food-bearing relative across the pond, just as chickens pursue a hen fortunate enough to have found a large grub.

On ponds and streams the striders stay close to shore or hurry to reach it if a breeze springs up and the water's surface becomes ruffled. In rain and in winter, the striders leave the water and crawl out upon the bank. In spite of these precautions, the insects do get wet at times. Although they show great ability in navigating streams, and can spring ahead to make progress against the current, an occasional bug is swept through a riffle and fails to stay afloat. In such situations, striders may be found below the surface film, rowing to shore where they can crawl out again to dry and clean themselves.

There is also a seagoing water strider—a small gray form common in tropical and subtropical lagoons and mangrove swamps, where it congregates in large groups. These same water striders are found at great distances from land, riding the waves like the best sailors. No one knows what they do during a storm at sea or when it rains. They must get wet, and what is there to crawl out upon to dry? To add to the problem, these seagoing striders often crawl down into the water during calm weather, and row along to feed there, upside down, on the underside of the water film. Many of them live out their lives hundreds of miles from shore and raise their families at sea. The eggs are laid on seaweed at the surface of the ocean or on the infrequent feathers dropped by sea gulls.

Spiders and mites of several kinds frequent the water film in pursuit of the insects there. They have the same means of staying dry as do the striders, and they scamper about on ponds picking up food wherever they can find it. Most of the water spiders are tan with dark stripes. Many of the females lay their eggs in a creamy sphere of silk, and drag this precious ball after them wherever they go, even out upon the water film. One of these ball-making spiders is a giant called *Dolomedes*. A full-grown mother may measure two inches or more between the tips of outstretched legs; her egg sphere may be

half an inch in diameter and contain hundreds of potential spiders. Even after they hatch, the spiderlets stay with their parent, and the adult is often seen with a fuzzy covering that can scurry off, like goslings from a mother goose. Such a family group is quite a prize for a hungry fish, and those spiders that hesitate while running on the water film may lose their lives. However, spiders seldom stop on the surface; they run from shore to plant or from leaf to lily pad, carrying their prey with them to a safe spot. But fish in ponds and streams follow walkers on the water to profit from their occasional unwariness. Sometimes a fish makes a mistake and seizes a spider's ghost—the empty, castoff skin. Often these skins float downstream, casting on the bottom a shadow much like that of the spider itself. But the skin rests on the water like a dead crane fly, while the living spider walks well above the surface with only its eight feet furrowing the film and making sharp silhouettes on the sand below.

Most conspicuous of the water mites is a common one with a ball-like body of brilliant, velvety red. The full-grown mites reach a diameter of a quarter of an inch. They run so smoothly on their very short legs that they seem to glide over the surface film rather than move on distinct feet. The females leave solitary brown eggs on floating vegetation, to hatch into immature mites with six legs instead of the characteristic eight. These larval mites spend a few weeks as parasites on some insect. They lie in wait for water striders or diving beetles—anything that comes their way. Sometimes they ride on damsel flies; more often they catch the striders. One strider may carry several of these clinging mites, each sucking nourishment, yet seeming to do little harm to its host. Eventually they drop off, molt to gain another pair of legs and the spherical form of the adult body, and forage for themselves as their parents do. The mites not only run along the surface, but frequently climb down plant stems into the water and swim about. Their eight short legs give them an even motion by which they may be distinguished easily from all other aquatic animals.

Very small insects with waxy feet can stand upon the water film without the additional precaution of spreading their legs widely. The smallest mites have this advantage. So do the several kinds of springtails and the many leaf hoppers that jump over the surface. These animals are so very light that even when they press down sharply on the water film to throw themselves into the air and escape some danger, they do not produce any sizable dimple in the surface. The leaf hoppers have leaping legs like a locust's or a katydid's, but the springtails have a much more ingenious way of catapulting themselves into the air. They are grotesque insects, with an extended tail. Some merely keep this tail curved under them, almost resting on the water film between their six short legs. To jump they simply straighten out, but do so suddenly. Others carry the tip of the underturned tail in a special catch, like the notched trigger of a mousetrap. The tail is strained against the catch just as is the mousetrap spring. When the insect is frightened (or sometimes seemingly just for fun), the catch is slipped, the tail whacks the water film, and the springtail is thrown high into the air, to land somewhere else. For a creature so minute, air has an excellent cushioning effect, so that the springtail settles without damage, usually on its feet.

Two types of springtails are common on fresh water and one on quiet bays of the ocean. The more abundant of the lake and pond forms is bluish black, about an eighth of an inch long. It congregates in such enormous numbers as to appear as a conspicuous blue-black band along the water's edge. The individuals walk about among their fellows, but at the slightest disturbance, the group flings itself into the air like tiny corn kernels popping on a hot griddle. They alight many inches away, no longer in association with one another. To all intents and purposes, they have vanished. Like other springtails, these have a tubular extension from the underside near the catch for the springing organ. With this "ventral tube" they are able to hold themselves to the water surface. The tube can be wet by the water and forms

a sort of anchor for the insect. The seagoing springtail and the seagoing strider are among the relatively few insects that are not bound to a land or fresh-water existence. The seagoing springtail is found all over the world along sandy and muddy coasts and on tidal pools in rocky shorelines. No one knows what happens to them during storms and showers at sea. Not all of them can come ashore for such occasions.

THE underside of the water surface is also used by a surprising number of aquatic creatures. Every now and again a pond snail crawls up a plant stem as far as the water surface, there to roll over and glide out under the water film, its flat foot pulsing with slow waves of movement from aft to fore along its length. In this position, many of the snails apply to the water surface a part of the body between foot and shell, and there open up the single hole that leads into the lung, so they can breathe in a load of air to take below. In very shallow water, a flat sole similar to that of the pond snail, but much smaller, shorter, and narrower, turns out to belong to a worm that is all sole, with almost no thickness. This flatworm, a free-living scavenger related to the liver flukes and tapeworms, is commonly called a "planarian" and seemingly has crossed eyes spotted on its speckled back. It is a source of never-ending delight to all biology students, and a laboratory pet with a firm grip on life. The animal is so elementally constructed that pieces cut from a single planarian can reorganize to form a whole. Biologists have worried out the philosophies of "self" in terms of many-headed, several-tailed planarians, which creep along the sides and water surface in laboratory jars to mock their captors.

Another animal capable of remarkable regeneration is *Hydra,* named two centuries ago by a man who, discovering that it could multiply heads if mutilated, thought of

POND SNAILS IN A SHALLOW PUDDLE

THE ONE WITH THE BLACK-SPOTTED SHELL IS ON THE BOTTOM ; THE OTHER CRAWLS UNDER THE SURFACE FILM.

the Greek mythical monster of that name. *Hydra* looks like a discarded umbrella without any cloth covering—merely a stalk with long arms from one end. The arms are tentacles with nettling cells for catching microscopic life, and between the arms is an opening into the animal's interior through which the prey is thrust for digestion. The opposite end of the stalk is armed with a sticky disk, the stickiness of which is under the creature's control. Often *Hydra* reaches a few of its tentacles upward and attaches them to some plant stem, then lets go with the sticky disk, to somersault in slow motion and glue its body to a higher point. Repetition of this process or a gradual gliding of the sticky disk may bring the animal almost to the water's surface. There *Hydra* often hangs, foot stuck to the underside of the water film, body pendant, tentacles outstretched for an inch or two beyond, waiting for unwary water animals to bump into its battery of stinging cells. *Hydra's* weight upon the water surface forms a dimple there, but the depression is not like the furrow under the water strider's foot. It is more like that around the snail or flatworm, and *Hydra* can creep along the water as they do, although with no visible waves of movement. These submerged creatures produce a water-repelling material from the flat area applied to the surface film. The water draws away, clinging wetly only to the rim of *Hydra's* disk or of the soles of snail or worm.

Two types of minute crustaceans upon which *Hydra* feeds have odd relationships to the underside of the water film. One of these, *Scapholeberis,* by name, has special waxy bristles with which to puncture the water film from below and lay hold upon it. Since these bristles are on the underside of the crustacean, the creature rests back downward, supported by the film. For purposes of camouflage its body coloring is related obviously to its upside-down position, for instead of being dark-backed and light-bellied like fish and most other animals, *Scapholeberis* is the opposite. In this position, the crustacean rows itself about with its long antennae, browsing on algae that float upward from below, and upon pollen and other

A MOSQUITO LARVA

THIS WRIGGLER HANGS FROM THE SURFACE FILM BY ITS BREATHING TUBE, WHICH HAS A WHORL OF WAXY HAIRS AROUND THE ORIFICE OF THE TUBE.

flotsam accumulated on the water surface from the air above. A gust of wind tows the surface water and *Scapholeberis* attached to it—a "sort of submarine sailing," some have aptly called it.

The other type of crustacean is typified by *Bosmina,* a tiny creature often trapped by accidentally breaking through the water film as it swims along below the surface. Unless a wave or similar disturbance knocks the helpless creature below the film again, it must wait in this position—partly in and partly out of the water—until it can molt its skin and slip out of the old covering into the lake below. The difficulties encountered when a small underwater animal is caught by the "dry" surface of water are similar to those experienced by animals of similar bulk and strength when they fall into the water from the air. Unless a branch is near by upon which they can crawl out, they usually drown or are picked up by water striders and other carnivorous creatures that make this two-dimensional world their home. Even water striders have difficulties; readers

of Frank E. Lutz' field book are cautioned to carry home their striders in a dry pail, not in water, lest they drown.

The many insects that live in ponds and streams must have atmospheric air to breathe, and remarkable provisions have been made for reaching through the surface film.

Back swimmers, mosquito wrigglers, diving beetles, and other water beetles come to the surface from time to time to thrust through the water film some tubular mechanism in order to replace the air stored beneath their wings or in their breathing tubes. Only by such frequent restocking can they carry on their precarious submarine existence. Some of the fly young, shaped like maggots and other peculiar things, have telescoping segments at their hinder ends, which they can extend to and through the surface film for gathering air continually while the creatures burrow busily to find food in the mire. Some of the water-beetle larvae not only come up to get their air but also drag living or freshly killed prey to the surface and thrust it out into the air, where it can give less resistance to being swallowed and where gravity can be of more help.

One of the water beetles has a greasy back, which repels all moisture like the feathers of a duck. This is the whirligig beetle, which passes much of its life at the surface of ponds and streams. Actually, it is a double animal—dry above and wet below—with paddlelike feet to propel it rapidly through the water. Even its eyes are divided into an upper portion for vision into the air and a lower part with which to watch the water's depths. These beetles are very vigorous swimmers, familiar to most people as they zig and zag along the water surface, commonly in groups, leaving behind them little Vs of waves like tiny speedboats.

The water film forms a definite barrier for insects that must lay their eggs in the water itself, and an almost endless variety of solutions to the problem can be observed. Perhaps the simplest is that of the water-lily leaf beetle, which cuts a small, circular hole in the dry top of its lily pad, pushes its abdominal tip through the hole into the water below, and while standing high and dry on a familar surface, lays two rows of eggs on the undersurface of the lily leaf in close concentric arcs. Many dragonflies, caddis flies, May flies, and others extrude a single egg or a group of eggs from the abdominal tip while soaring over the water. They fly down close and suddenly flick the abdomen through the surface film and liberate the egg. Pulling quickly out, they zoom away to repeat the process when the next egg is ready. The insect's momentum ensures it against being caught in the surface film and dragged in to drown. Much more careful are the ordinary biting mosquitoes, which literally lay a raft of eggs. The raft floats upon the water film with only its lower surface wet. The eggs hatch through their lower ends and the young wrigglers emerge into the water directly, many to be eaten at once by hungry beetle larvae and fish. The malaria mosquito, in contrast, lays her eggs singly on the surface of the water.

Another little fly, the *Dixa* midge, stands on the surface like a water strider while she deposits an extruded wet mass of eggs suspended by a strand of gelatine. As the egg mass is let down into the water, the fly adds to the supporting filament a circular, transparent disk that repels water. The disk catches on the surface film and pulls down a dimple as the strand below lengthens out and the suspended eggs sink farther into the water. The fly leaves, but the eggs with their little float drift around as the water surface is blown or as currents move the water itself. The eggs may become stuck to some vegetation or break their mooring and sink to the bottom, there to hatch.

Some other insects, when ready to lay their eggs in the water, wrap their wings around their bodies like a cloak, enclosing a bubble of air, and then crawl down stones or stems through the surface film and into the depths below. Those that succeed in laying their eggs and escaping capture in the water may later crawl back out into the air, dry off, and fly away. Male and female damsel flies cooperate in this. The male uses a pair of claspers at the end of his long abdomen to hold the female by her slender neck. After

her eggs are fertilized and seem ready for laying, the pair alight at the water's edge and the female backs into the water down some stem. The male holds on to her and remains above the surface at least as far as his wings. When the eggs have been deposited, the female starts upward and the male pulls, fluttering his wings, so that between their combined efforts, the female is brought out of the water again, to dry off and fly away.

THESE many special abilities and difficulties related to the water film are based on the very high surface tension so characteristic of this commonest liquid. Each adaptation in form or behavior is a means of using or of circumventing the strong surface forces involved. None of these methods do anything to change the surface tension itself. Yet this is possible, and is the basis of a familiar parlor trick. If a chip of gum camphor is dropped into a dish of water, the chip spins around and sails in erratic courses, propelled by a mysterious force. Actually the camphor is dissolving faster along some parts of the water line than in others, and since camphor greatly reduces the surface tension of water, the surface forces are weakest where the gum is dissolving most rapidly. The chip moves because of the inequality of surface forces pulling it from all sides. The weakest forces are behind the moving chip and show where the camphor is dissolving fastest.

One of the rove beetles makes use of this trick. Like its relatives, *Stenus* is an active little beetle, running or flying around much of its time in search of carrion or prey small enough to overcome and eat. Sometimes *Stenus* falls into a puddle or a pond. It has no waxed hair to keep its feet dry and therefore sinks well into the surface film, its legs and underparts thoroughly wetted by the water. But *Stenus* merely expels from its anal glands a substance that makes the water wetter—reducing the surface tension at its posterior end. Undiminished surface forces in front of the beetle promptly draw it forward. As long as the insect continues to emit this magic substance, it sails along with no apparent effort. Often the beetle reaches some dry object upon which it can crawl to dry off and again take to flight. This rove beetle can keep up its speedboating for many minutes, but if it is deprived of the abdominal tip with the anal glands, the insect is quite helpless in the water.

The leaves of some of our pond plants are like the whirligig beetles, with a water-shedding top and a lower surface that rests in, and is wet by, the water. Lily pads are of this sort, anchored to their loglike roots by long slender ropy stems. Their two surfaces support two different types of clinging life, one wet, one dry, while in between the water lily gathers up the sunshine energy to make it grow. The duckweeds too, small flakes of green, are always at the water surface. The larger kinds rest on the surface with rootlets dangling into the pond below. One of these duckweeds is our smallest flowering plant, floating freely just below the water film among the lily pads. Contrast with this the largest water leaf of the Victoria lily, whose six-foot disk with turned-up rim will float a human child of medium size, all safe and dry.

When spring comes to the lake or pond surrounded by pine-clad hills, the water film takes on a golden yellow cast with squandered pollen grains. The wind makes patterns of the driven dust, while the whirligigs cruise through it and raise a wake like tiny boats, rocking all the water bugs and giving rise to quiet lappings on the near-by shore. Spent and useless to the trees around, this pollen dies and sinks below. Throughout the years it may build up a layered record of the past. From just such fossil pollen we know many of the plants which lived and passed away but left no other mark. Pollen, wind, and water film combined to make a fossil trail of bygone trees.

NATIVE ROCKS AND MINERALS AS FERTILIZERS

W. D. KELLER*

Dr. Keller worked in professional nonmetallic and petroleum geology and as a ceramic technologist until he took his Ph.D. in geology at the University of Missouri in 1933, after which he became assistant professor of geology at that school. He taught at the U.S. Army University in Florence, following Italy's military collapse, and returned to Missouri as professor of geology.

THE need for the addition of inorganic (mineral) plant nutrients to soils is continually becoming more acute. The most obvious warning is the decline in productivity of soils impoverished by extractive farming, but other warnings, such as malnutrition of animals fed on plants growing on deficient soils, are appearing. Every farmer since the days of Columbus has recognized by shrinking crops when he has "mined out" his soil and has been ready to abandon the old place and stake out a new claim. We are now meeting ourselves coming back from the search for virgin soils and must give thought and energy toward conserving and maintaining the geological heritage that is in the soil.

In principle, the return is simple, but in actual practice some serious problems arise. The very magnitude of the soil requirements poses an astounding problem of the adequacy of their sources. Moreover, the mineral or chemical compositions of the materials available for addition and their relative solubilities become highly significant. The problem is both pedological and geological. In this article I shall consider the thesis that future replenishment of soil minerals will come advantageously and predominantly from relatively crude native rocks and rock-forming minerals that are available in sufficiently large quantities to meet our needs, and that selected native silicates, or blended silicates, phosphates, and carbonates will be added, as well as processed salts.

* The author is indebted to his colleagues in the Soils ond Geology Departments at the University of Missouri for their aid and close cooperation in the preparation of this article. Professors Albrecht and Graham, in particular, furnished data and helped repeatedly by constructive criticism.

ANY of or all the mineral fertilizers needed by soils might be discussed with profit, but only one example, that of potassium, will be considered in any detail. Where, for instance, can we obtain sufficient potassium for all our present soil needs—and the continuing and mounting needs? The answer is not a simple wave of one hand toward the Texas–New Mexico evaporite basin and the other toward Stassfurt. Second, is a processed potassium salt (muriate, for instance) the logical, natural, and best form in which to add it, or should not the major addition of potassium be in the compounds similar to those from which the soils have evolved? Both these questions invite consideration from the geological point of view.

In 1944, a year of record potash production up to then, 652,260 short tons of K_2O were delivered for agricultural uses in the United States. This is a large and truly laudable production, but when spread over actual needs it dwarfs to almost microscopic proportions. At least half the tonnage was taken by only eight states (Florida, Georgia, Illinois, Maryland, North Carolina, Ohio, South Carolina, and Virginia); what can we expect the other forty to do about meeting their needs?

The Southern states need enormous amounts of potash for improved yields. A recent writer recommends for the development of good pasture land in Mississippi an initial application of 200 pounds of 50 percent muriate of potash per acre and a subsequent annual addition of 50 pounds. The entire production of potash for 1944 would have treated initially only about 13,000,000 acres, less than half Mississippi's area (probably about the area of its crop land), leaving none for the rest of the country.

Moreover, continued farming rapidly depletes everywhere the potash reserve our soils have inherited. For the year 1944 we took out of our soil in corn (grain) 454,880 tons of K_2O, 169,761 tons of K_2O in wheat, and 771,084 tons of K_2O in alfalfa.*

We hauled away in alfalfa more K_2O from our soil in 1944 than the entire potash fertilizer delivered! The combined corn and wheat grain, not counting the fodder and straw, took out almost an equal amount. Obviously, this exhaustive withdrawal from the soil, without replenishment, cannot continue long without very serious results.

Three possible means of replacement of soil potash reserves are apparent: (1) provide it as commercial fertilizer to supplement that weathered annually in the surface soil; (2) let nature bring it in with wind, flood water, glacial ice, or landslides; and (3) let weathering of the subsoil or subjacent parent rock make available virgin potassium-bearing minerals to a descending soil system.

Although the wind does move many tons of soil each year, it blows away, as well as adds to, any one soil plot, and the net result of gain from wind action, broadly viewed, is practically nil. Except for local stream flood plains and alluvial fans, the additions to soil by water, ice, and gravity are chiefly of academic interest. Item (2) of the above three can therefore be dismissed.

With reference to the release of potassium by the weathering of subjacent rock, the rate of this may be determined for average conditions with moderate accuracy. The K_2O content of an "average" limestone (composite analysis of 345 limestones) is 0.33 percent. If this limestone were to supply 106 pounds of K_2O for an acre of alfalfa, about 32,121 pounds of the limestone would need to weather per acre per year. Using a figure of 160 pounds for the average bulk weight of limestone per cubic foot, and an area of 43,560 square feet per acre, the 32,121 pounds of limestone would be equivalent to a thickness of 0.0046 foot over the acre; that is, the average limestone will need to weather to a depth of 0.0046 foot in one year, or 1 foot in 218 years, to replenish the soil in K_2O taken out, assuming that no K_2O is lost by erosion. But an estimate of actual chemical denudation (the way limestone weathers) in the United States, excluding the western Great Basin (arid) region, places the rate at only 1 foot in 23,984 years (and 1 foot in 30,000 years for the land of the earth).

Hence, for the average data taken for all aspects of this problem, in order for weathering of parent underlying limestone to replenish its "rich limestone-derived soil" in K_2O when alfalfa is grown thereon, weathering will need to be accelerated to *one hundred times* the present normal rate!† The additional loss of K_2O by soil erosion places additional demands on replenishment. Return of potash by barnyard manure from animals fed on the alfalfa will compensate for part of the potash extraction, but this return is far from being adequate to replace the amount removed.

The potassium contributed to the soil by the addition of agricultural limestone is very slight because that limestone is usually selected for purity and high content of soluble Ca (and Mg in dolomite). In passing, probably we should revise our practice and add limestone containing "impurities" rich in potash and/or phosphate minerals.

* Data on production from A. C. Britton, statistician, U.S.D.A., Columbia, Mo.

3,203,310,000 bu. corn on 97,078,000 ac., av. 33 bu./ac.
1,072,177,000 bu. wheat on 59,095,000 ac., av. 18 bu./ac.
31,863,000 tons alfalfa on 14,548,000 ac., av. 2.2 tons/ac.
7.1 lbs. K_2O/25 bu. corn amount to 9.4 lbs./av. ac. corn.
3.8 lbs. K_2O/12 bu. wheat amount to 5.7 lbs./av. ac. wheat.
48.4 lbs. K_2O/ton alfalfa amount to 106 lbs./av. ac. alfalfa.

† Average limestone would have to weather 0.000408 foot in thickness annually to replace the K_2O taken out by corn.

$$\frac{9.4 \times 100}{.33 \times 43{,}560 \times 60} = .000408$$

It would have to weather 0.0002475 foot in thickness annually to replace the K_2O taken out by wheat.

$$\frac{5.7 \times 100}{.33 \times 43{,}560 \times 160} = .0002475$$

Chemical denudation is about .000417 ft./yr. (1/24,000).

Sandstones contain on an average 1.32 percent K_2O; an average shale contains 3.24 percent K_2O; and an average igneous rock of the United States, 3.23 percent K_2O. From the standpoint of composition, they should be somewhat better soil replenishers of K_2O than limestone, but experience from cropping soils overlying the noncarbonate rocks does not indicate any significant relief by natural weathering from shortage of potash reserves.

Potassium (and other fertilizer elements) may be added to soil by man following two procedures. One is the present conventional practice of adding, in a commercial fertilizer, a selected few moderately pure, relatively concentrated elements in salts of high solubility to supply quickly the mineral-derived substances (usually potassium, calcium, and phosphorus) most acutely needed. This may be thought of as dosing the ailing soil-patient with a quickly assimilable, concentrated, processed, stimulating food to revive its strength.

Another procedure to restore soil reserves might be termed the natural, long-term, geological scheme. Hereby would be added, in clay, silt, and fine-sand sizes, carefully chosen, and perhaps blended, mixtures of rocks and minerals that would weather and decompose rapidly in the soil from a geological viewpoint, but slowly compared to reactions in the laboratory; these would supply a long-lasting, full-diet, soil builder. The elements and plant nutrients liberated would be considerably more diverse than those of the more nearly pure, processed fertilizer, and would include those needed in minor quantities, such as Mn, Zn, Cu, Co, and B. The intention would be to fertilize as nearly as possible according to the principles followed by nature in soil building.

The use of the conventional commercial fertilizer puts emphasis on high concentration, quick availability, addition of minimum bulk, and deliberate selection of a few elements. Many well-known advantages accrue from this practice; but, over a long period, some disadvantages may result from the very features that have appeared beneficial for a short time. Long-continued use of the chloride salt (muriate of potash) will bring about increased concentration of the chloride ion in the soil, a factor that is well recognized as affecting adversely the germination of seeds. Moreover, general experience has not favored a chloride-rich soil. Then, too, the advantages of high solubility and quick availability of a commercial fertilizer make for short life and the necessity of repeated application.

Another serious defect of a highly soluble, concentrated fertilizer is the powerful mass-action effect that it exerts to overstock with a few elements the humus and clay colloids from which the plants accept their nutrients, thereby suppressing, or blotting out entirely (for practical purposes), the availability of other elements also sorely needed by plants for optimum growth. The rich solutions from the soluble fertilizer convert the "nutrient jobber" colloids into a relatively homogeneous, undiversified system, which forces onto the plants an excess of a few elements to the exclusion of others. By its overconcentration in some constituents such a fertilizer creates nutrient deficiencies in others—despite its purpose to correct deficiencies. Albrecht clearly pointed out the advantages of heterogeneity in the soil system when he wrote:

> These facts support the concept that the soil need not be a uniform medium as to degree of acidity or as to the distribution of all the essential plant nutrients. Rather, the soil may be a mixture representing a heterogeneous collection of foci of each of these in the mineral or rock forms weathering slowly while in contact with the acid clay. Plant growth may then represent the summation of root contacts with all these different centers of fertility as the roots move to and get from them all that is needed for maximum crop productivity.

His lucid description of probably normal plant growth and search for nutrients is definitely contraindicative of an artificial dosing of the colloids. Instead it describes a mechanism compatible with the nutrition-deriving experience of plants through their eras of evolution, the utilization of rock and mineral particles as ultimate sources of nutrient:

> It seems more reasonable to believe that the plant is growing much better if its advancing roots find one area around a limestone particle that may be

nearly neutral but providing much calcium, then at some distance another area more acid, where iron can be taken, then another where around some feldspar its potassium is slowly made available, then another where the acidity is mobilizing the phosphorus, and still others where manganese and the different essential nutrient elements are on the clay in exchangeable form because the clay is in contact with some mineral fragment and maintains itself in a weathering equilibrium with it.

The deleterious effects of calcium overdosage because of excessive additions of limestone that has been crushed too fine may be prevented by use of coarser particles, which dissolve and release calcium more slowly. Satisfactory release may also be had by applying natural calcium silicate or artificial metallurgical furnace slag, which decomposes more slowly and releases calcium accordingly. The advantages of the slow-acting calcium compounds point the way to similar advantages from other slow-acting native mineral fertilizers.

The need for the minor elements such as boron, zinc, copper, and manganese may not be supplied in the proper natural balance by man's too-narrow, selective, artificial replenishing. Plants have been evolving a long time by feeding on a diet wide in variety of elements available. Eventually we shall discover the proper concentrations of each element needed and synthesize the perfect fertilizer, but while these investigations are maturing, a "shotgun" application of the original natural food, the primitive rock, is logically indicated.

The primitive geological rocks and minerals, through which evolution of the plant world has progressed so far, most probably were composed of the silicates like those occurring in the igneous rocks available for quarrying today. In general, the igneous rocks contain a wider variety of minerals than do the sediments that are differentiates, or "purer and refined" concentrates, of quartz, carbonates, and clays (and the soluble constituents of the ocean). Consequently, the igneous rocks are to be looked to as the more nearly balanced and complete inorganic plant food, and as a source for a soil replenisher, and sedimentary minerals as special, more concentrated supplements.

GROWTH ON VOLCANIC SOIL

GRAPES AND PINES GROWING IN THE LOOSE CINDERS AND DUST ON THE SIDE OF VESUVIUS. VERY LITTLE WEATHERING HAD TAKEN PLACE IN THIS VOLCANIC MATERIAL. PHOTOGRAPHED BY THE WRITER IN 1945.

Proven practical testimony supporting the case for the efficacy of igneous rocks lies in (a) the rich soils rapidly converted from volcanic rocks—for example, at Vesuvius, in Mexico, some parts of the northwestern United States, Hawaii, and Java—and (b) the fully nutritional plant foods grown in our Western states on soils that contain a rich silt reserve of igneous rock minerals.

The quick conversion of Vesuvian basaltic volcanic dust and cinders to a singularly productive soil is classic. In Hawaii the basalt-derived soils, which still contain a reserve of basalt not completely decomposed, release potassium over an astoundingly long interval of extraction. The volcanic soils of Java supply food for a population density of over 900 per square mile, in contrast to the approximate density of 100 per square mile on near-by Borneo, whose soils are leached lateritic types with no recent volcanic activity.

The properties of these rich parent rocks that are significant to soil development are that the rocks are mafic (name derived from

magnesium and ferrum) to intermediate in type, containing abundant calcium and significant potassium, rather than silicic (like those containing quartz or free silica); that they contain notable amounts of glass; and that they are fragmental to a greater or less extent.

Mafic rocks are readily oxidized (Fe″ to Fe‴) and hydrated and therefore are susceptible to rapid weathering. Where they are glassy, they are highly susceptible to hydrolysis; if fragmental, they will possess thin edges, angular shapes, and porous packing—all of which operate, with the mafic composition, to convert the rock quickly to an early and highly productive soil. Artificially crushed, glassy to fine-textured rocks, even though not naturally fragmental, should respond much the same way, and pedologists may well take the hint from Mother Nature and duplicate her work.

Laboratory experiments have shown that relatively pure silicate minerals hydrolyze when ground in distilled water. Graham found that silt-sized mineral fragments in contact with acid clay hydrolyze and release potassium, calcium, sodium, and magnesium. Simultaneously, hydrated aluminosilicates or, possibly, hydrated alumina and/or silica are freed. A new base-exchange medium is immediately established upon their breakdown. The plant food from the silicate minerals is delivered slowly, at a natural soil-forming rate, from *within* the soil body, and not as a deluge of outside intoxicant.

With reference to the rate of release, Graham showed that within 107 days, 3.4 percent of the total calcium of anorthite feldspar had been taken into solution. Those data extrapolated to 100 percent weathering indicate that the silt particles of anorthite would be completely altered by a highly acid soil within about 10 years. This figure is not to be taken too rigorously because the rate of weathering will be modified by the acidity of the soil, the extent of leaching by ground water, the action of plants removing Ca from the clay, the effect of CO_2 and humic acids, the temperature, other cations present, and diminishing size of the weathering silt particle. However, the 10-year figure is probably a good indicator of the order of magnitude of the length of time that anorthite silt can remain in, and contribute to, the soil. It indicates that this calcium feldspar is not exhausted in 1 year or that it would not last 100 years, and it is not out of line with our experience of calcium availability of soils under actual cropping conditions.

Graham's valuable pioneer work needs to be supplemented by working in the laboratory with a greater variety of minerals than he covered in his first experiments and by testing rocks that, as well as containing the minerals and elements desired, are available for quarrying and transporting economically to areas of soil need. Field tests of the response of impoverished acid soils to additions of selected crushed rocks and minerals may well be run along with laboratory work.

Pedologists and geologists should cooperate to test and supply the most readily available and practical raw materials within various regions. It is important that the mineralogy and petrography of the rocks to be tested should be considered. For example, potassium-bearing orthoclase and microcline feldspars in the Missouri red granite apparently are not as effective sources of potassium as is leucite, the potassium feldspathoid in Vesuvian lava, or the lava itself. Therefore, simple reference to the chemical composition of a rock does not furnish adequate information on its properties. In this respect, Ross and Hendricks point out, in a discussion of the Piedmont soils: "Here then is an excellent example where the course of weathering is determined by the mineral composition of the rock rather than by the overall chemical composition." Minerals of the olivine, pyroxene, and calcic plagioclase groups are most susceptible to weathering attack.

A choice of rocks and minerals that would most likely be best as mineral fertilizers will be a compromise between the ideal mineral composition, the ideal texture, availability, and adequate quantity. A list of such rocks, with their chemical compositions, occurring widely separated, is given in Table 1.

The rocks numbered 1, 4, and 5 are from regions whose highly productive soils were developed readily from the rocks; therefore,

TABLE 1

COMPOSITION (PERCENT) OF ROCKS POSSIBLY SUITABLE FOR USE AS FERTILIZERS

	1	2	3	4	5	6	7
SiO_2	48.10	52.23	44.40	49.73	55.06	46.04	49.69
TiO_2	1.41	2.27	1.53	3.05	0.36	0.64	0.85
Al_2O_3 ...	17.56	11.22	10.95	16.39	17.92	12.23	18.06
Fe_2O_3 ...	2.48	3.38	5.15	7.58	4.39	3.86	2.64
FeO	6.10	1.84	2.77	3.98	5.24	4.60	6.19
MnO	....	....	0.08	0.23	0.50	Tr.	0.13
MgO	4.27	7.09	1.75	4.06	3.26	10.38	5.73
CaO	8.16	5.99	8.49	7.17	8.29	8.97	8.24
Na_2O ...	2.65	1.37	6.50	4.12	3.09	2.42	2.99
K_2O	7.93	9.81	8.14	1.93	2.01	5.77	3.90
H_2O+ ...	0.12	....	1.17	0.54	0.07	....	....
		2.65	....	....	....	2.87	0.91
H_2O- ...	0.04	....	0.24	0.81	0.23	....	....
P_2O_5	1.01	1.89	0.37	0.84	....	1.14	0.81
ZrO_2	Tr.	....	0.03	0.03	....	....	....
BaO	0.08	....	0.01	0.03	....	0.48	....
SO_3	....	0.74	0.06	....	....	Tr.	....
F	....	0.50	....	....	....	....	....
Others ...	....	1.68	0.12	....	....	....	....
Total ...	100.82	100.62	100.76	100.53	100.42	99.76	100.27

1. Vesuvian lava, 1903. ROSENBUSCH-OSANN. *Elements der Gesteinslehre.* Stuttgart: E. Schweizerbartsche.
2. Wyomingite, Leucite Hills, Wyo. SCHULTZ, A. R., and CROSS, WHITMAN. *U. S. Geol. Surv. Bull. 512,* 11.
3. Arkite, Magnet Cove, Ark. WASHINGTON, H. S. *J. Geol.*, 9, 1901, 616.
4. Basalt, Mauna Kea, T. H. WASHINGTON, H. S. *U. S. Geol. Surv. Prof. Paper 99,* 1917, 522.
5. Pyroxene andesite, Java. WASHINGTON, H. S. *U. S. Geol. Surv. Prof. Paper 99,* 1917, 512.
6. Leucite basalt, Highwood Mountains, Mont. CLARKE, F. W. *U. S. Geol. Surv. Bull. 770,* 1924, 462.
7. Basalt, Table Mountain, Colo. *U.S.G.S. Bull. 770,* 460.

they are examples of proven desirability. The high K_2O content (7.93 percent) of No. 1 resides mainly in the feldspathoid mineral, leucite. A pyroxene mineral contributes Ca, Mg, and Fe oxides. No. 2, which occurs in Wyoming, also carries its K_2O (9.81 percent) dominantly in leucite, with pyroxene, amphibole, mica, and dense matrix material furnishing abundant Ca, Mg, and Fe oxides. It is logical to expect the rocks from the Wyoming Leucite Hills to respond in a soil much as the Vesuvian lava does. A large quantity, proven reserves, is exposed to view in Wyoming. Schultz and Cross computed in 1912 the following minimum:

Leucite-bearing rocks in tons	1,973,496,177
Potash (K_2O) in tons	197,349,617

These figures do not take into consideration the rock that occurs in the dikes associated with the volcanic necks and surface flows, nor the material underlying the mesas which has not been estimated in the discussion of the separate exposures.

Perhaps some of the demand for K_2O can be shifted from the Texas–New Mexico region to Wyoming.

Leucite-bearing basalt, No. 6, and other alkali-rich rocks occur in great quantities in the Highwood Mountains. Minerals composing No. 6 include leucite, pyroxene, olivine, biotite, analcite, and apatite. In the Bearpaw Mountains of Montana occurs pseudoleucite tinguaite, which contains 9.81 percent K_2O. Montana also possesses adequate reserves of potash-rich rocks.

In the Magnet Cove, Arkansas, region occurs arkite, No. 3 in the table, which is rich in potassium. It is composed of leucite, nepheline, orthoclase, pyroxene, and garnet. The analysis shows considerable Ca as well as determined amounts of Mn, Ba, Zr, and

other minor elements. Other alkali-rich rocks occur in association with arkite.

Rocks No. 4 and No. 5 do not show particularly high K_2O content, but they weather to some of the most productive soils known. No doubt their occurrence in a climate favorable to a high rate of weathering is a significant factor, but still they serve to demonstrate that basaltic rocks do furnish the elements needed for plant nutrients. The basalt, No. 7, from Table Mountain, Colorado, compares favorably with those from Hawaii and Java. Basalts are exceedingly widespread in distribution, and, because they vary in mineral and chemical composition, they offer a number of possibilities for soil additions. Each individual basalt should be examined on its own merits.

Besides the primary rocks discussed, there are other rocks and minerals that are logical sources of potash. Glauconite is a sedimentary, complex, hydrated potassium iron silicate with proxying Na, Mg, and Al, which occurs in abundance in the green sand of New Jersey, in some Gulf Coast formations, and in certain older, lithified sediments such as the Bonneterre dolomite of Missouri. Green sand averaging about 6 percent in K_2O content could be produced from large reserves. Glauconitic dolomite, besides furnishing potassium, would supply also calcium and magnesium, and the quarrying and crushing of it would cost no more than for ordinary limestone.

A dolomite, the Waterton formation, which contains about 40 percent by volume of authigenic glassy orthoclase feldspar in grains 0.01–0.05 mm. in diameter, occurs in large quantity (at least 60 meters thick) in the Waterton Lakes region, Montana–Alberta. Daly reported the chemical and mineral analyses for it given in Table 2.

Although current work indicates that igneous orthoclase furnishes potassium very slowly to soil colloidal clay, the fact that the Waterton feldspar is authigenic in the dolomite reopens the possibility that it might be more responsive to weathering. What an attractive limestone it would be, *if* it proved to be effective.

Alunite and alunitized rock, such as occur in concentrated deposits and in altered rock associated with epithermal ore deposits in Utah, California, Nevada, Colorado, Arizona, and elsewhere, can furnish up to a theoretical 11.4 percent K_2O. Alunite is a sulphate, $KAl_3(OH)_6(SO_4)_2$, the sulphate radical being of questionable desirability. Alunite is sparingly soluble in its natural occurrence, but its space lattice is broken down by heating to 700°C.–800°C., after which the potassium sulfate dissolves readily. In commercial practice, the alunite would probably be calcined at the mine, to save shipping the combined water, and crushed to desired size. Possibly calcination should be only partial in order to slow down the solubility of potash. Along this line, consideration should be given to calcining other earth products for the purpose of increasing the solubility of certain of their constituents. Heating of mixtures—for example, lime with a silicate—may improve the rates of availability of both. Although heat processing

TABLE 2

COMPOSITION OF A DOLOMITE (PERCENT)

SiO_2	30.46
Al_2O_3	6.86
Fe_2O_3	4.53
FeO	1.89
MgO	10.07
CaO	16.02
Na_2O	0.38
K_2O	5.77
H_2O	1.42
CO_2	22.55
	99.95
Sp. gr.	2.749

The estimated composition of the rock (assuming the iron oxides to represent magnetite) is, by weight:

Orthoclase	34.5
Albite*	3.1
Quartz	6.0
Magnetite	6.3
$MgCO_3$	21.2
$CaCO_3$	28.6
	99.7

* The albite is probably in solid solution with the orthoclase. So far as known by published analysis, no other non-metamorphosed dolomite or limestone even approaches the Waterton dolomite in its abundance of the alkalies; feldspar makes up about 40 percent of its volume.

costs money, and the product will have to compete with other materials whose composition or location enhances their value, heating may still be profitable.

Emphasis has been directed toward the mineral and chemical compositions of rocks that are potential sources of mineral fertilizers, without much comment on the quantity of rocks available. One exception has been the tonnage occurring in the Leucite Hills. The volumes of rocks exposed in the Highwood Mountains, the Bearpaw Mountains, Magnet Cove, and basalts in Colorado and elsewhere are to be reckoned in terms of cubic miles rather than in tons. Concern is not about quantity of these rocks, but proximity and means of transportation to regions of need. The problem of supply is parallel to that presented by agricultural limestone today; it is practical availability, not scarcity of "ore." Long shipping distances may be feasible under some specialized conditions. For instance, if Vesuvian cinders were used as ship ballast, that material might be shipped on an economical basis as a by-product to Florida, where it could be spread on those thin, sandy soils. Whereas a highly soluble commercial fertilizer has to be applied perhaps three times a year, and is leached away from the sand, the weathering Vesuvian rock would leach very slowly. Instead, it would make available a concentration of nutrient elements normal to plant development, and would weather most probably to a clay of the montmorillonite-beidellite family having a high base-exchange value. The Vesuvian soil is productive at home in grapes and citrus fruits; Vesuvian cinders could be expected to perform equally well in furnishing a fairly complete range of nutrients to Florida citrus groves.

Whereas the reserves of the potassium "ores" of the New Mexico–Texas area are more definitely circumscribed in quantity and location, the use of crushed native rock as a potash replenisher opens wide many other possibilities. The continued demand for

THE BOAR'S TUSK

A VOLCANIC PLUG IN THE LEUCITE HILLS OF WYOMING. THE NAME APPEARS APPROPRIATE. WYOMINGITE OCCURS IN THE BOAR'S TUSK, BUT THE GREATER QUANTITY IS PRESENT IN THE LAVA FLOWS THAT TOP NEAR-BY MESAS.

highly soluble potassium salts is anticipated to remain so high that even with the use of silicate rocks our richest evaporite reserves will be depleted too soon.

Efficient use and development of native rocks for soil replenishment calls for close cooperation between soils specialists and geological surveys or institutions to classify rock deposits on bases of mineral and chemical composition and tonnage of rock that can be quarried, crushed, and transported economically. Compromises on these factors may provide the most mineral nutrients per dollar spent. Possibly a locally occurring, low-cost, but partly deficient rock may be advantageously blended with small quantities of another of higher cost to give a better-balanced mixture at lower cost than if only one rock were supplied. Individual regions with their particular needs and resources will require individual solutions. This discussion can only point out generalities.

Promptness, immediacy, or speed of response by impoverished soils to native rock additions will not be as early as to quickly soluble fertilizers. One or more years of weathering will elapse before the ions from the rocks will become available to the plants. Just as raw rock phosphate is slower in action than acid superphosphate, so will the silicate rocks be slow to get started.

A thorough addition of a balanced rock mixture should last several times as long as an equal application of quickly soluble fertilizer, and at no time would there be any effect of overdosage or overconcentration. A rock supplement lasting 10 years (utilizing Graham's data) would be cheap even if it cost, say, three times as much as a soluble fertilizer that had to be applied each year.

Decline of fertility as the rock fertilizer became used up would be gradual, much as our fertility is declining now, rather than an almost catastrophic slump such as occurred in certain European districts after war cut off their fertilizer supplies. Subsequent maintenance additions of rock fertilizer could be made at the physical and financial convenience of the farmer without an immediate press of necessity. The strategic value to our nation of longer-lasting fertilizer is obvious.

It may be as shocking financially as the appearance of new taxes to press the point and insist on hauling tons of rock onto the soil, but, when we face squarely and quantitatively the measure of soil fertility (mineral nutrients), we know that every pound of mineral matter hauled off the land in crops is extracted from the soil, and that it can be replaced in only two ways: from the subsoil or subjacent rock, or by being carried in. Either we extractively mine the soil, or we replace what we take out. If we maintain or restore our soil mineral heritage, we must haul back pounds of rock (soil mineral builders) for pounds of crops hauled away to urban districts. We cannot thrive on carbohydrates alone, plant-synthesized air, water, and sunshine, whether or not we like the idea of carrying rock back to the soil. The inescapable fact is that we will be forced to replenish soil minerals. Logically, geologically, and pedologically, and from the standpoint of quantitative adequacy of reserves, eventually the native rocks must be utilized to furnish most of our mineral fertilizers.

METABOLISM AND BIOENERGETICS

OLIVER P. PEARSON

Dr. Pearson, who completed this work at the Biological Laboratories at Harvard, is now with the Museum of Vertebrate Zoology, University of California, Berkeley.

THE most fundamental physiological property of all plants and animals, the rate at which they are living, is reflected quantitatively in their metabolic rates. Metabolism is a complex, inscrutable process—but its rate is easily measured. Heretofore, such measurements have been used chiefly by physiologists as tools for studying the functioning of organisms; I shall endeavor to show that they can also be used by ecologists and philosophers in understanding the energy balance of the earth, and that a person could even, through measurements of metabolic rates (and here I exaggerate only slightly), relate every beat of a mouse's heart to the entropy of the cosmos.

Consider the mouse represented in Figure 1. He takes in food, water, minerals, oxygen, nitrogen, and many other essential, helpful, or useless materials, but almost everything is returned to the environment, as shown in the segment of Figure 1 marked "Borrowed and Returned." Some substances, like the nitrogen of tidal air, are borrowed for only a moment; an indigestible seed might be borrowed for several hours, and a milligram of calcium for a fleeting lifetime, but all are returned to the environment to play a part in the lives of other animals or plants. The only permanent loss to the environment is the energy dissipated in the form of heat, represented in the third segment of Figure 1. The mouse has taken a bit of solar energy (captured and made edible by plants) and released it. Perhaps we should say the animal has squandered it, because the energy has been degraded to such an extent that it is beyond recall by the plant and animal kingdoms. It is this energy degradation that is measured as the metabolic rate.

As long ago as 1886, Boltzmann (quoted by Tizard, 1932) remarked that the struggle for existence is a struggle for free energy available for work. Therefore, to know the metabolism of an animal for a day is to know how successful it has been in the struggle for existence, how successfully it has deprived some rival of energy, how much of its organic heritage it has reduced to impotent isothermality.

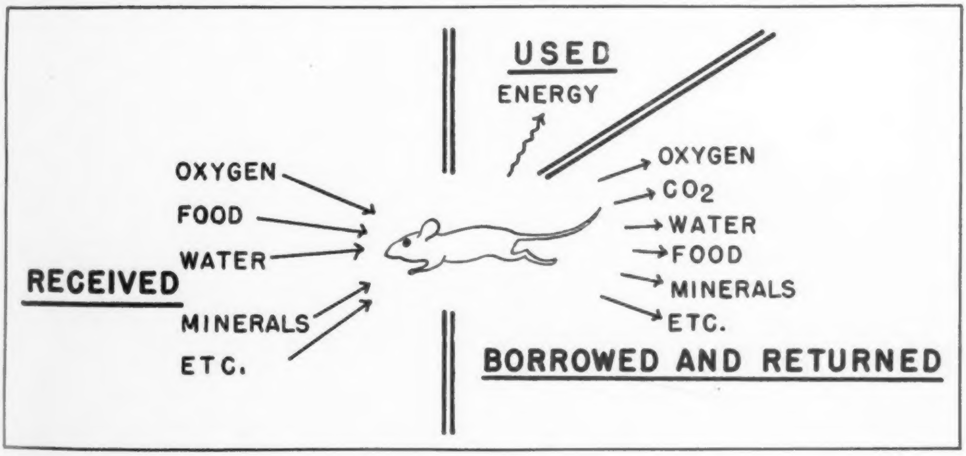


FIG. 1. INTAKE AND OUTPUT OF A MOUSE

Since Mother Nature's ability to keep her cupboard supplied is limited by the earth's meager quota of sunshine, since many animals and plants are making use of the cupboard quite freely, if not recklessly, and since I am somewhat fearful that this pilferage may become so rampant as to make it difficult for my friends and me to satisfy our needs, I have decided to expose some of the culprits.

Pearson (1947) and are not basal rates, but are average daily rates taken over 24-hour periods at a temperature of about 25° C., with the animals free to move about and express their normal nocturnal-diurnal rhythms. The figures for population per acre are estimates based on personal trapping experience in the northeastern United States and on many recently published population studies.

TABLE 1

THE ENERGY DISSIPATION OF SMALL MAMMALS ON AN ACRE OF FOREST IN PENNSYLVANIA IN SUMMERTIME

	Ave. wt. (gm.)	No. per acre	Gm. per acre	Metabolic Rate	
				Kg. Cal./kg./day	Kg. Cal./acre/day
Short-tailed shrew (*Blarina brevicauda*)	18.0	4.0	72.0	600	43.2
Deer mouse (*Peromyscus leucopus*)	20.0	5.0	100.0	416	41.6
Red-backed mouse (*Clethrionomys gapperi*)	22.0	0.5	11.0	414	4.5
Woodland jumping mouse (*Napaeozapus insignis*)	24.0	0.5	12.0	360	4.3
Flying squirrel (*Glaucomys volans*)	70.0	0.1	7.0	231	1.6
Long-tailed shrew (*Sorex cinereus*)	3.5	0.2	0.7	1,800	1.3
Total		10.3	202.7		96.5

The rate of living of many citizens has been measured and recorded in the papers. Consequently, I can calculate the extent of their thievery from the public energy supply and can state definitely that big, boisterous Smith, because of his flagrant metabolism, is Public Enemy No. 1, whereas little, mild Miss Jones is only a Lesser Public Enemy. But I can go further. I can group all the Smiths and Joneses together (for not one of them has a clear record) into a genus, *Homo,* and compare their thieving, their dissipation of this limited supply of energy, with that of a host of smaller public enemies—the mice, for example, or the earthworms.

Table 1 might be considered a rogues' gallery of the records of most of the common small mammals living in forests in eastern Pennsylvania. The animals are listed in order of their metabolic (or catabolic) importance. The metabolic values are taken from

It will be seen that short-tailed shrews and deer mice are metabolically the most important species. Deer mice are more abundant than shrews in the forest, but since they have a lower metabolic rate they are no more important. The long-tailed shrews, despite their astonishingly fast rate of metabolism, have little effect on the total energy exchange of the area because they are so small and so few. The total impact of all these small mammals from the Pennsylvania acre considered in Table 1, all 203 grams of them, is reflected in a metabolic rate of 96 kg. Cal. per acre per day.

This metabolism may be compared with that of other kinds of animals living in Pennsylvania (Table 2). Deer, for example, which are unusually abundant in the state, dissipate the energy in vegetation at a rate of 70 kg. Cal./acre/day—about three-quarters as fast as all the small mammals listed. This

calculation is based on the following assumptions: 1,100,000 deer in the state (Biological Surveys Inventory, 1943), average weight of 100 pounds, and an average daily metabolic rate of 40 kg. Cal./kg./day (twice the basal metabolism of goats).

To introduce man into Table 2, I have used the density of Indians rather than that of modern man because Indians lived in closer harmony and more exact balance with their environment than does man today. In the year 1600 there was about 0.00054 Indian per acre in New England, New York, Pennsylvania, and New Jersey (calculated from data given by Mooney, 1928). At a daily metabolic rate of 2,500 kg. Cal. per person, their total metabolism would have amounted to only 1.4 kg. Cal./acre/day—about the same as that of the smallest mammal of the region, the long-tailed shrew. Modern man, however, is living in Pennsylvania at a density of 0.3 persons per acre, which results in a metabolism of 750 kg. Cal./acre/day—nearly seven times greater than that of all the small mammals listed in Table 1, and 535 times greater than that of the Indians. Even when we consider a less-urbanized state, such as New Hampshire, with a present population density of 0.09 per acre, the energy dissipated by man is about 225 kg. Cal./acre/day, which is nearly double that of all the smaller mammals put together.

Actually, man is an even more expensive item in the energy budget than these figures indicate, for he has been able to supplement his own metabolism by putting fire to work. I shall not attempt to calculate exactly how much of this energy is totally wasted and how much is "saved" by charging batteries, heating houses to lower man's metabolism, etc., but let us suppose that each person finds it necessary to burn one ton of carbon each year, and that all the resulting energy "goes up the chimney." All this supplementary oxidation would drain 1,765 kg. Cal./acre/day from New Hampshire and 5,980 from Pennsylvania. With this outside help, man's impact on the environment, his bioenergetic importance, becomes much greater than that of any other mammalian species—probably greater than that of all other mammals combined. For many years he has been drawing on the earth's capital to support this high living; most of the other animals live frugally within the earth's income.

Having found that modern man is the greatest energy squanderer among the mammals, let us see if he has any rivals among other groups. We are faced at once with a shortage of population and metabolism data, but we can make some rough estimates. Using the method described by Eaton and Chandler (1942), I have estimated that there were 125,000 earthworms (25,400 gm.) to an acre of hardwood forest at Swarthmore, Pennsylvania, on June 10, 1947. At 19° C. these would use oxygen at a rate of 0.07 cc./gm./hr. (Loewy, 1926), or 1,778 cc./acre

TABLE 2

DISPOSITION OF THE ENERGY FALLING ON AN ACRE IN PENNSYLVANIA IN SUMMERTIME*

	No. per acre	Gm. per acre	Metabolism (Kg. Cal./acre/day)	
Used by man (Indians)	0.00054	25	1.4	
Used by deer	0.04	1,700	70.0	
Used by small mammals	10.3	203	96.5	
Used by earthworms	125,000.0	25,400	205.0	
Used by microorganisms	3×10^{18}(?)	3×10^{6}(?)	2,700.0	
Total used by above animals			3,072.9	3,073
Energy still available to other organisms				12,927

* Rate of incident radiation, 25,000,000 kg. Cal./day; rate of capture by plants (net productivity), 16,000.

/hr. At an equivalence of 4.8 kg. Cal. per liter of oxygen, this would be 205 kg. Cal. /acre/day. The worms on this acre, therefore, would be about twice as important metabolically as the mice and shrews, but less important than "barehanded" modern man in Pennsylvania.

If we go to the bottom of the size scale and consider the microorganisms in the soil (mostly bacteria), we find that on an acre such as the one postulated in Table 2 they are the most important of the groups listed. Metabolically, they are more than ten times as important as the earthworms. (I have used Russell's figures [1937, p. 459] for the energy dissipation of microorganisms on unfertilized land.)

We must now compare the rate at which these organisms are depleting the supply in Nature's cupboard with the rate at which it is being stocked—that is, the rate at which plants are capturing and storing our quota of energy. The energy serving to warm the earth and atmosphere is not being considered here, for although an unknown amount of it is "conserved" by lowering the metabolism of birds and mammals, at the same time it increases the metabolism of the cold-blooded animals. During Indian times in Pennsylvania, plants captured about 16,000 kg. Cal./acre/day of the 25,000,000 kg. Cal. that bathed each acre daily. I have assumed that all the land was forested and had an annual net productivity of 200 tons of organic carbon per square kilometer (Noddack, 1937). In Table 2 this income is compared with the expenditures of the few groups of animals that I have mentioned. It may be seen that these animals dissipate 3,073 of the 16,000 calories made available each day, leaving some 12,000 available for the birds, bees, toadstools, and a host of others, or for storage. This storage is temporary, however. More energy is captured each day during the summer than is used, resulting in the storage of some, but almost all seems to be used up over the winter. Geologists support this belief that there is little energy being stored in forests today, and Russell (1937) states that the soil microorganisms live right up to their income in the matter of nutrients and energy supply.

In the preceding pages I have only outlined an approach that should yield interesting results when it is applied to different kinds of terrain at different seasons with *all* the animals and plants carefully accounted for. I have not undertaken this task, for, finding unexpectedly that my friends and I with our cars and furnaces and washing machines are by far the greatest of the public enemies, I have had a twinge of conscience, a resurgence of altruism, and am preparing to retire to a life of studied inactivity and heat conservation under the whispering leaves of the nearest palm tree.

REFERENCES

EATON, T. H., JR., and CHANDLER, R. F., JR. The Fauna of Forest-humus Layers in New York. *Cornell Univ. Agric. Exp. Station Memoir 247*, 1942, 1–26.

LOEWY. Sauerstoff-Verbrauch von Kaltblütern. *Tabulae Biologicae*, 1926, **3**, 503.

MOONEY, J. The Aboriginal Population of America North of Mexico. *Smiths. Misc. Coll.*, 1928, **80**, 1–40.

NODDACK, W. Der Kohlenstoff im Haushalt der Natur. *Zeit. ang. Chem.*, 1937, **50**, 505–10.

PEARSON, O. P. The Rate of Metabolism of Some Small Mammals. *Ecology*, 1947, **28**, 127–45.

RUSSELL, E. J. *Soil Conditions and Plant Growth.* (7th ed.) New York: Longmans, Green, 1937. Pp. i–viii, 1–655.

TIZARD, H. T. Thermochemistry. *Encyclopaedia Britannica*. (14th ed.) 1932, **22**, 83–91.

FOR NOW AND THE FUTURE

ROSS T. McINTIRE

Admiral McIntire, wartime Navy Surgeon General and White House physician for President Roosevelt, was appointed director of the National Blood Program on July 20, 1947. A native of Salem, Oregon, Dr. McIntire attended the University of Oregon and received his M.D. from Willamette University in 1912. He entered the Navy in 1917.

THE turn of the new year marked the beginning of one of the greatest and most far-reaching undertakings of this century in saving human life. The new National Blood Program of the American Red Cross is under way—an adjunct to medical science and a step forward in national preparedness.

The wastage of human lives in war—and in peace—is so obvious that it requires little comment. However, when we find that war has developed new lifesaving methods, these are worthy of further study for the use of mankind during the times of peace.

Blood might not be considered a new lifesaving agent. In a way this is true, but in World War II we learned how to use whole blood in entirely new ways. The men in the field used daring methods in their efforts to save lives. At Tarawa, where so many of our marines lost their lives, hundreds owe theirs to the use of plasma, serum albumin, and whole blood. Four-way transfusions, giving plasma and whole blood simultaneously through both arms and legs to men who had lost a major part of their own blood, were performed with success. Those men are alive today.

The American people gave freely of their blood so that our fighting men might have a better chance of recovery, regardless of their injuries. In the Navy and Marine Corps, the record of lifesaving showed that only 2.37 percent of the wounded failed to survive. We can give a great deal of credit for this extremely low mortality to the blood and its derivatives that were supplied in abundance to our armed services all over the world.

New medical findings are being reported constantly. Today, one of the broad fields of research is in human blood. What is it? What are its functions? What makes it work? The latter two basic question have been answered, oddly enough, more fully than the first one. We know that blood carries food to the tissues, acts as the transportation system for waste products, and circulates the hormones, enzymes, and many chemically active substances needed to regulate body functions. We know it is a circulating fluid, deriving its main propulsion from the heart, nature's most effective pump.

But the question, What is it? though the one logically asked first, is the one last answered. We know much of the answer, but not all.

Blood is a highly complex fluid that ties all parts of the body together. We can separate the solids, the red cells, from the liquid part, the plasma. Between World War I and World War II, a sound method was devised for drying the plasma and, by proper packaging, preserving it for use over a period of years. It proved an excellent substitute for whole blood, for it could be easily transported and did not require complicated storage facilities.

Believing that plasma itself contains substances of great value in saving life, Dr. Edwin J. Cohn, of Harvard University, devised an ingenious method for fractionating plasma and producing from it serum albumin. The Surgeon General of the Army and I had an opportunity to work with the National Research Council in putting into effect the studies made by Dr. Cohn and his associates. Consequently, from Pearl Harbor on, there was never a lack of those lifesaving substances.

There was more to be found when scientists probed further.

As dramatic as the story of how plasma saved thousands during the war is the story of blood plasma fractionation. Just as crude oil is broken up into countless useful products, plasma may be separated into as many as sixty constituent parts. Some of these fractions have already found their niche in medical science, but the secrets of many others are still locked within them. Through research, we are diligently seeking their uses and their applications to modern medicine. Though research work on plasma is still in its infancy, the wealth of knowledge we now have is serving the physician, the surgeon, and the public-health officer.

In breaking down the plasma, one of the first fractions drawn off is the substance that causes the blood to clot. Two products of great and practicable value are being made from this fraction: antihemophilic globulin and fibrinogen. Antihemophilic globulin supplies the substance lacking in the blood of hemophiliacs, those unfortunate persons commonly known as "bleeders." The blood of such persons does not clot normally, and they often bleed to death from a minor cut or other wound. Antihemophilic globulin, injected intramuscularly, makes it possible to perform even major surgical operations on hemophiliacs without undue risk, for the blood can temporarily be made to clot normally. It is hoped that this plasma fraction eventually may be used by hemophiliacs in much the same way that sufferers from diabetes administer insulin to themselves hypodermically.

Fibrinogen, the long needlelike molecules that combine with thrombin, another substance of the blood, to form blood clots, is also found in Fraction I. This is a most helpful aid to surgeons in controlling bleeding. It is this product of Fraction I that has the widest use.

Fraction II of plasma contains in concentrated form the antibodies of the blood, both the natural ones and those built up through recovery from certain diseases. Most prominent and widely used product from this fraction is immune serum globulin, used to modify and prevent measles. For this purpose, it is the best therapeutic agent known today. Measles, though a common disease usually associated with childhood, can be very dangerous. Its danger lies in the possible aftereffects, such as fatal pneumonia, a weakened heart, and damage to the eyes and ears. Administered in proper dosage after exposure, immune serum globulin allows a mild case of measles to develop. This generally builds up a lifelong immunity, at the same time preventing a serious case from which complications may ensue. When necessary, in the event a patient already ill is in a weakened physical condition, this plasma fraction can prevent the disease entirely. Preliminary investigations give some hope that immune serum globulin may be effective in preventing complications of mumps, especially in adults. Similarly, investigation in the use of this product to combat infectious hepatitis suggests that it may be effective in modifying and preventing this disease. It is imperative, however, that further investigation be carried out before this globulin can be used in general medical practice for mumps and infectious hepatitis.

The third plasma fraction contains the isoagglutinins of the blood. To give whole-blood transfusions safely, blood must be typed according to the four groups: A, B, AB, and O. Concentrated agglutinins, prepared in a stable form, can be used to type whole blood quickly, providing the best-known agent for accurate blood grouping. From the isoagglutinins of selected people, we can obtain the very necessary serum for determining the Rh factor The discovery a few years ago of the Rh factor in blood has resulted in safer use of whole-blood transfusions. In addition to blood grouping and cross-matching, we now know that the blood to be used for transfusion purposes must be compatible with that of the recipient.

Also from Fraction III comes thrombin. When thrombin is combined with fibrinogen (found in Fraction I), fibrin is the result. Fibrin is a powerful hemostatic agent. From fibrin, two very valuable products can be made: fibrin film and fibrin foam. As its name denotes, fibrin film is an almost transparent, cellophanelike substance which, when

soaked in a thrombin solution, is pliable and can be applied to the contours of the tissue. Surgeons find it excellent material in replacing the dura of the brain, and of tremendous help in nerve surgery. Fibrin foam, a sponge-like product derived from whipping the liquid fibrin into a foamy mass and allowing it to dry, is soaked in thrombin to give it hemostatic action. Both these fibrin products are of distinctive importance: They can be left in an incision and, being pure blood protein, will gradually redissolve into the blood stream without causing adhesions or other harmful effects. It has been found that skin grafting can be performed more successfully by the use of this combination of fibrinogen and thrombin.

Fraction IV can be divided into many subfractions. In these, we find globulins that make it possible for iron and fat molecules to combine and be assimilated by the body.

The largest percentage of plasma, almost half, is serum albumin, Fraction V. Serum albumin contains proteins of the blood that maintain osmotic pressure in the blood vessels, thus preventing the liquid portion from seeping out into the body tissues, as happens in cases of shock. During the war, processed serum albumin was packaged in a stable liquid for use as a plasma substitute where space conservation was necessary, particularly in planes and submarines. Its competency in preventing shock is about five times as great as an equal volume of plasma. Knowledge we have of serum albumin opens new avenues for the treatment of certain kidney diseases, such as nephrosis. No doubt one of the major uses of serum albumin will be in cases of hypoproteinemia, where the protein of the blood has been greatly decreased. This condition may result from numerous diseases. Serum albumin is also being used to great advantage in supplying digested proteins to the blood of premature infants. Albumin as a substitute for blood offers advantages over plasma, but in many accident cases and in burns, whole blood is needed as soon as possible.

There are still other fractions of blood plasma that warrant continued study and clinical investigation. This research is currently being done, and there is evidence that many portions of plasma will find extraordinary uses in medical practice.

The red cells, too, are the subject of much scientific study. We know more about them now than was known during the war, when millions of pints of these cells were washed down the drain in the preparation of plasma for the armed forces. They are no longer being discarded. Already under way are studies that will increase the knowledge we now have. In many patients with various types of anemia, there is an inadequate supply of red cells, but these patients have adequate plasma in their blood streams. By concentrating the red cells in the same amount of liquid volume, it has been found that the red blood corpuscle count can be restored to normal in a shorter period of time than would be possible with whole blood. Like whole blood, these red cells centrifuged from the plasma can be put into a preservative solution and with proper refrigeration can be kept usable for as long as twenty-one days.

Red cells can be dried and ground to a powder, or they can be allowed to remain in a thick liquid, or paste. Their use on vascular ulcers and wounds that are stubborn in healing has proved very helpful.

Research on the red cells has not stopped; it is still being carried on, as are the studies on the plasma fractions.

Many of these findings are the results of research intensified during World War II. These are the things we know human blood can do in surgery and in preventive medicine. The question now is, Where and how can these advances be applied to peacetime medicine? Blood is a rare commodity. There is no satisfactory substitute for human blood.

Though we have emphasized the importance of the component parts of blood, we must also point out that the greatest use of blood is for whole-blood transfusions. Blood is amazing in its ability to restore life where blood loss has occurred through accidents, certain diseases, childbirth, surgery, and burns.

Where to get enough blood, and how to bring it and its derivatives within the reach

of anyone who needs them, were the problems facing physicians after the war. Even in localities where hospital and medical facilities are unusually good, it was difficult to procure enough blood. Blood derivatives such as described here were next to impossible to procure, and, if they were obtainable, they were so expensive that only a limited few of the nation could afford them. The blood plasma declared surplus by the Army and the Navy, returned to the Red Cross, and distributed through state departments of health to hospitals and physicians throughout the country is being used up more rapidly than had been expected. That supply will be exhausted about the end of this year. There was no national planning to provide these products of blood and whole blood itself, needed in ever-increasing quantities. In the event of a national disaster, we would be unprepared.

These problems were being given serious thought. Along with the thinking, tentative plans were formulated to determine whether such a project as a national blood program could be undertaken. The Red Cross, through its Advisory Board on Health Services, composed of eminent authorities in every phase of medical and health fields began to consider whether it should, would, or could carry out such a task.

After months of careful consideration, exploration, and consultation with the many national organizations that would be concerned with such a program, as well as with Red Cross chapters, the decision was made that the Red Cross organization would undertake a National Blood Program. It was discussed with, and approved in principle by, leaders in the American Medical Association, the American Public Health Association, the American Hospital Association, the Army, the Navy, the Veterans Administration, the United States Public Health Service, the American Dental Association, and the Association of State and Territorial Health Officers. Each group was aware of the need for a national blood program and each agreed that the Red Cross, with its broad wartime experience in collecting blood on a large scale, was the organization best suited for the job. In June 1947 the final decision was made by the Board of Governors of the American National Red Cross.

What the National Blood Program will do is to furnish blood and blood derivatives for medical use throughout the nation. There will be no charge for these products, for they will be supplied by the American people themselves, from their gifts of blood. These products will be made available for anyone who needs them, and the only cost to the patient will be the fee of his physician or hospital for their administration.

This is a vast program, one that will require time to be put into effect throughout the entire country, one that will require continuing cooperation on the part of all the American people. It will not be an easy task. Nor will it be an inexpensive one. But it will benefit, directly or indirectly, every person in the country.

In its operation, the National Blood Program must be sufficiently flexible to meet widely varying needs and conditions in large and small communities. It must be carried on with the support of the medical profession, hospital authorities, and the duly constituted health officials.

The problem of obtaining sufficient blood and blood products for wide use will be of far greater magnitude than was the one of providing blood for the military. In order to derive the maximum advantages for all the people, at least two things are necessary: one, to broaden the base of blood procurement, processing, distribution, and research; the other, to organize these activities so that whole communities may participate. These two requirements are interdependent. Broadening the base geographically to include a population of many millions makes it possible to build up dynamic public support, to collect blood economically, to apply mass-production methods to the processing operation, to employ technical specialists, and to ensure a steady flow of blood. The need of organization for such a project is obvious enough. We must not fail to realize that not only are the hospitals and physicians concerned, but that the general public is a very necessary partner.

The National Blood Program may be divided into four distinct phases:

1. Collecting the blood.
2. Processing it for use as whole blood and blood derivatives, including packing and storage.
3. Distributing the blood and blood products for ready availability to physicians and hospitals for the needs of the people.
4. Making blood available for continuous research and investigation to ensure safety of the products and to determine the uses to which they may be put for the greatest benefits.

Collecting blood will follow two procedures: through permanently established centers in large communities; and the use of mobile units that will periodically visit outlying communities, taking with them the equipment and technical personnel required. Suitable quarters will be provided for both types of centers. Medically competent personnel—physicians, nurses, technicians—will staff these centers and mobile units; within the centers volunteer nontechnical assistance will be needed. Red Cross chapters will have the responsibility of organizing their communities to ensure the best possible support in this continuing program. When the project is in full operation, it is expected that approximately 3,700,000 pints of blood per year will be collected.

Processing the blood will require highly skilled work and intricate machinery. Some of the blood collected will be examined, typed, and distributed to local hospitals for use as whole blood, for which there is the greatest need; when available some will be shipped to commercial laboratories equipped to produce the derivatives. Contracts will be made with reliable concerns by the Red Cross national headquarters for the production of derivatives.

It must be remembered that whole blood is a perishable product, requiring careful handling to avoid wastage. Facilities for the rapid distribution of whole blood to hospitals and laboratories must be available. In each community both whole blood and the derivatives will ultimately be accessible to physicians and hospitals for their patients. Not only civilian hospitals, but those of the Veterans Administration, the Army, the Navy, and the hospitals of the United States Public Health Service will be included.

Specialists throughout the country will be supplied with blood and blood derivatives for research purposes and clinical investigations, to make sure that the products are put to the best use. This controlled research will be under the guidance of the Blood and Blood Derivatives Committee of the Red Cross Advisory Board on Health Services, and in collaboration with the National Research Council. The program will bring to medical practice the advantages of new discoveries and new blood derivatives as they are developed.

It is of course imperative that the highest of standards shall be maintained—in the technical work; in the methods for collection, storage, and distribution; and in the investigative field.

This vast program will require large sums of money for its operation. However, the costs will be less as a national program than would be many individual community or state-wide programs. Five million dollars may be needed during the first year and perhaps fifteen million when the program expands to serve the entire nation. The cost of this comprehensive blood program will be borne by the Red Cross, which receives its contributions from the people. No separate fund drives are contemplated; the money is expected to come from funds collected during the annual Red Cross fund campaign. Approximately ten cents per capita is a small price to pay for a national blood program. Yet there is no way to measure the value of lives saved, diseases prevented, and the general improvement of health.

Initial operations will begin in six localities. These have been selected for the advantages they offer geographically and strategically for the program's beginning. These first centers will open in Rochester, New York; Washington, D. C.; Louisville, Kentucky; Atlanta, Georgia; Wichita, Kansas; and Stockton, California. Others will follow as quickly as time, personnel, and equipment will permit. Gradually, this expanding program will reach every community in the United States.

Several communities have shown foresight in planning to meet local needs for blood. Three state departments of health are at present conducting blood programs with Red Cross chapter aid—Michigan, North Dakota, and Massachusetts. Massachusetts has the only complete program, i.e., providing whole blood and the derivatives. Communities that have been considering the establishment of local programs might well proceed with their plans. They will be a step ahead, for eventually it is hoped that such programs may be integrated with the National Blood Program. In this way, full benefits from the fractionation phase of the project will be available to all.

Ahead lies a tremendous task, essential to peacetime needs and national preparedness.

THE ROADS

From my window I see many roads,
Many narrow roads pass by my house,
Highways old are they.
They are silent roads though much used,
No sounds of going do I hear from them.
I never see a traveler there,
No passers-by on any of them,
Yet they are busy roads.
Words travel on these roads—
Words from the rich, a lengthy caravan,
Words from the poor, a little company,
Words power laden,
Words sorrow laden,
Words joy laden,
Swift as the light,
Pass north, south, east, west.
Wonderful! These many narrow roads,
These busy, silent roads
That pass my house.

PAUL B. JOHNSON

IMPRESSIONS OF INDIA*

ALBERT F. BLAKESLEE

Dr. Blakeslee (Ph.D., Harvard, 1904) has had a long and distinguished scientific career. In 1904 he went to Europe as an investigator for the Carnegie Institution; in 1915 he became its resident investigator for plant genetics and then held the posts of assistant and acting director of the Department of Genetics, finally in 1936 becoming director of the Department, which post he held until 1941. He has taught at Harvard, Radcliffe, and the University of Connecticut and is now director of the Smith College Genetics Experiment Station. Dr. Blakeslee was president of the A.A.A.S. in 1940.

THIS is a report of our trip during the winter of 1946–47 to the Indian Science Congress in Delhi, to which Dr. Wm. Edwards Deming and I were delegates from the A.A.A.S. With the exception of Dr. Shapley, who arrived after the Congress had started, the American and Canadian delegates left La Guardia Field December 22 on a BOAC Constellation four-engined plane for an eleven-hour flight to Shannon Airport in Ireland.† There, while we had an early breakfast, we learned a lesson in nationalism from the labels over the various doors—they were in Erse as well as in English and French. Questioned, all the attendants confessed, however, that they could not speak the Irish language, although it is now a compulsory subject in Irish schools.

In London we waited a week for a chartered plane. This took us and the British delegates to Karachi, India, with stopovers at Malta, Cairo, and Basra. An Indian government plane then took us from Karachi to Delhi. Delayed by fog at the London airport, we were a day late in arriving; in consequence, the Congress opened its sessions one day late.

After the Congress, for the most part in the company of Dr. W. F. Hanna, of the Rust Research Laboratory of Winnipeg, I visited the Forest Institute at Dehra Dun, universities and scientific institutions at Lahore, Lucknow, Benares, Calcutta, Darjeeling, Hyderabad, Madras, Bombay, Jodhpur, and Karachi. Each of us gave one or more lectures in most of these cities. I left India by plane on February 9 and, after short stops in Paris and London, arrived in New York on February 18 on the *Queen Elizabeth*. Some of the delegates had shorter, and some longer, stays in India.

I shall not attempt to give a detailed report, but shall instead report some impressions of India gleaned from an all-too-hurried visit to that interesting country.

INDIA is faraway from us in customs and taboos, near though it is in time (thirty-four actual flight hours). It is a land of great contrasts—in climate, races, languages, religions, and types of government. To us the greatest contrast was the extreme poverty of the masses and the comfortable, even luxurious standards of living of the upper classes. As an example, we found people sleeping in the streets and on the steps of the great Mosque of Delhi because they had no other place to go; later, we were lavishly entertained in the Viceroy's sumptuous palace in New Delhi and were given a banquet by a wealthy industrialist, where we were served various wines and champagnes. Delegates to the Congress came in contact primarily with the university people and with the governing classes, both Indian and English.

In the Science Congress we found the meetings of various scientific sessions much like those of our own American Association for the Advancement of Science The dis-

* Contributions from the Department of Botany, Smith College, New Series, No. 21.

† The Canadian delegates were W. F. Hanna, T. L. Tanton, and R. B. Thomson. Those from the United States were A. F. Blakeslee, W. E. Deming, E. N. Harvey, Oscar Riddle, and Harlow Shapley. Those from England were P. M. S. Blackett, William Brown, Sir Charles Darwin, Sir Arthur Fleming, Munro Fox, L. J. Mordell, Sir Harold Spencer-Jones, Dudley Stamp, and Sir D'Arcy Thompson. V. P. Volgin and E. N. Pavlovsky represented the Soviet Union; Jacques Hadamard represented France; and S. S. Chern, China.

cussions were keen, and the criticisms of the papers were searching We were all impressed, I believe, with the knowledge the Indians had of foreign literature, and I had an experience similar to others in finding that my own work was well known to the specialists in my particular field.

It was a surprise to many Americans to learn that the Indian Congress was conducted entirely in English, although there are several hundred more or less distinct languages or dialects spoken in India. Those in the north trace their origin to the Sanskrit. The Dravidian languages in the south have an entirely different origin. English, in consequence, serves as a lingua franca and is spoken fluently by all educated Indians. With the exception of the two Moslem universities, it is the language of instruction in all Indian institutions of higher education. The value, at least to science, of a single universal language in which all educated men can converse is obvious. It was with some concern, therefore, that we found among some of the university people in India a desire to develop a single, national Indian language to take the place of English, although even Hindustani would have to be learned as a foreign language by a large proportion of the people. Perhaps this feeling was both an expression of nationalism and a reaction against anything connected with the English. Possibly our desire to see the spread of English as a world language fostered rather than restricted is somewhat influenced by our own nationalistic instincts, but I believe I should feel the same way about French or German if either were as widely used and appeared to have a better chance of general adoption.

India attributes to us many wonderful things for which we should not have credit. For example, an able woman botanist, in discussing the economic value of algae, said that in India they ought to do as we did in the United States, where every farmer had a fish pond in which he put certain chemicals to induce the growth of algae. Fish fed upon the algae, and the farmer could go out and bring in a crop whenever he desired! Some of the American delegates had heard of this scheme, but none of us had ever actually seen such a fish pond.

Indians believe that we do everything by mechanical contrivances, and, when told that in our homes we had to get along very largely without servants—of which there is an abundance in India—they would reply, "Oh, yes, but you do everything in your houses by push buttons."

Indian students also look to America. We were besieged by students who wanted our assistance in securing admission to American universities. They even called at our rooms before breakfast. One boy who wanted to come to the United States to study atomic physics was only fifteen years old.

The Indians with political influence look to America. For instance, Nehru, who invited Dr. Shapley and me to dinner one evening with two Indian scientists, was very eager to learn the attitude of Americans regarding our foreign policy, more particularly in its relation to Russia. He asked especially about the effect of the Republican victories in 1946 on our policies. We were greatly impressed by Nehru's keen interest in, and understanding of, movements in other countries and by his efforts toward the betterment of his own people. Although Indians of all classes look to American and Western civilization, they do not wish to be placed in the position of an inferior people asking alms. They resent the "master-race" discrimination exercised toward them both in India and in South Africa. In retaliation, they have established a boycott against African goods, and in the Taj Mahal Hotel in Bombay there are signs reading: "No South Africans Allowed."

The British also depend upon America. In January and February, 1947, when we were in India, the British government had not yet set a date for leaving India, but all the British officials realized that the government would soon have to be turned over to Indians. The British expect America to give at least moral support in helping India to get on its own feet.

The Indians have a civilization of which they are justly proud, for its origin may be traced back to Sanskrit times, some of the literature in the Vedas going back to about 2000 B. C. I became convinced that they are not mentally inferior, as we were led to believe before going to India and as was also

the impression one would gain from discussions with the British governors of India. The Indians realize that they must depend on Western civilization for their science and technology. I feel, however, that the differences in their scientific and technological outlook are due primarily to environment rather than to genetic constitution; hence, they are more amenable to change.

In my talks with Indian educators I pointed out that up to the early part of the present century we had to look to Europe, especially to Germany, for leadership in science, and that it is not so long ago that America (and Europe) came to realize that this was no longer necessary. In the same way, the Indians realize that they must get foreign training, and they have established fellowships for their best students to study abroad. Whatever shortcomings in colonial policy may be blamed upon the British, there was evidence that they had done much for the development and application of Indian science, at least in medicine. Many Englishmen we met had devoted the better part of their lives to scientific service in important posts in India, and some of them were seeking positions in other countries in view of the early withdrawal of English control. It is to be hoped that the value of their service will not be entirely lost, and that standards may even be raised now that India has taken over complete control of her own destiny.

The British in India have said that as scientists the Indians are most successful in mathematics and theoretical physics. This seemed to be true, although Raman, who received the Nobel Prize in physics, was and is a good experimentalist. The Indians, however, do not seem in general to do so well in experimental sciences. There were, of course, some notable exceptions, and in some of the laboratories we visited there was a keen spirit of research.

The greatest fault I could find with Indian scientists was a certain disinclination to use their hands. This fault I have also found among some other foreign scientists. The lack of appreciation of the dignity of manual labor, as well as the caste system—which now appears to be in process of gradual elimination—is a great handicap to their technological advancement. For example, an Indian who, when I met him, was scientific and technological adviser to one of the Indian states, told me that when he was a student in one of the institutes of technology in the United States he did as many of the other students did; namely, earned money during the summer in manual labor, washing dishes in a hotel, etc. When he wrote his parents what he was doing, he received two cablegrams telling him to stop it at once—that he was disgracing his family. The same scientist told me that he had available an industrial position at 1,200 rupees (about $400) per month, but that if he paid someone that salary he felt the man would not want to work in the shop, but would wish merely to sit at a desk. Perhaps as valuable a thing as any that I told Indian scientists with whom I had conversations was that I helped my wife wipe the dishes in our own household. The willingness to do—even at times the necessity of doing—things with one's own hands would be one of the best lessons, I feel, that could be learned by Indians who visit our country.

One of the American vice-consuls in India told me that the difference between Indians and Americans could be demonstrated by the fact that, when an American gets a Ph.D. degree, that is the start of his going to work more intensively, but when an Indian gets a Ph.D., that is likely to be the end of his work—he feels he has reached his goal.

I did get the impression that there was in many cases a letdown in the scientific activity of Indians who had reached a full professorship in their universities. I also found that the position of a man seemed to be more esteemed than the work he was actually doing. In one or two cases, the men who appeared to me to be doing the most original work in certain institutions had considerable difficulty in getting to me to talk about their investigations. In fact, I sometimes had the feeling they were actually being kept from me.

That "pull" is considered of much value in advancement was suggested by several requests made to me that I mention the work of certain Indians when I gave my lecture at their institutions. Another example is a letter recently received from a student who had failed to secure a fellowship from the

Indian government to study for a doctor's degree in the Genetics Experiment Station at Smith. He intimated that influence was important and suggested that I write to certain government officials urging them to reconsider their decision not to award him a fellowship. We heard other complaints that on account of political and personal influence Indian fellowships did not always go to the best students. Whether these complaints were sometimes justified or were merely the reactions of disappointed candidates, we were not able to judge. Naturally, however, I did not write the letters requested.

I should like to be a maharaja, not because of the great wealth and multiplicity of wives or their equivalents, with accompanying luxurious living, but rather for the good that I might do by means of science and its application. We were impressed with the need of, and opportunities for, intelligent philanthropy, both public and private. We had the privilege of visiting a few institutions established and maintained in whole or in large part by private donations. Among these were the Bose Institute for Plant Research in Calcutta, the Tata Hospital in Bombay, and the Sir Shri Ram Industrial Institute in Delhi, the dedication of which we attended during the Congress. Such institutions, however, appear to be relatively rare. Wealthy industrialists are not so numerous in India as with us, and in general they seem not to have acquired the habit of philanthropy in support of science and education.

Many prominent Indians have had British titles conferred upon them. These are highly esteemed despite the feeling in some quarters that their bestowal has been an attempt to buy allegiance to the British crown. If I were a member of the Indian government, I would suggest that India take over the conferring of titles, but on a revised basis adapted to her own country. I would suggest the title of "Indian Benefactor" for those who had made significant contributions to the welfare of India in art, literature, science, or philanthropy. Such titles might correspond to Nobel Prizes, but on a broader basis. A man so honored might write after his name the initials I. B. (B. I., "Benefactor of India," might be confused with "British India.") He might be addressed in the new Indian system as Benefactor Iziid Singh or, more simply, Bene Izzid, instead of the English system of Sir John Doe, which is usually simplified to Sir John. Such a system of nobility might emphasize the fact that service to one's own people is most worthy of recognition.

THE frank criticism I have made regarding some things that I feel are imperfections among the Indian scientists could be made also of many of our own American institutions. I came back from my trip, however, with a great enthusiasm for our Indian friends and a strong feeling that India is a land of great potential power.

The scientists and students we met appeared very appreciative of our visits and lectures. I myself feel that I got more than I gave in adult education. The trip was undoubtedly of value to both the scientists of India and those abroad in fostering international understanding. This, I believe, can best be done through science, for there is only one science.

THE COUNSELOR SAYS "M-HM"*

DANIEL MALAMUD

Mr. Malamud, born in Russia in 1921, received his M.S.S. from the New School for Social Research in 1942. He did clinical psychological work at Norwich State Hospital, Connecticut, until, in 1945, he entered the Army, where he administered group therapy at the Fort Knox (Kentucky) Rehabilitation Center. In 1947 he was appointed research director of the Central Harlem Street Clubs Project at the Welfare Council of New York.

"IT WASN'T what I expected at all, He just listened to me talk, said 'M-hm' a good deal of the time, didn't give me any advice or anything like that, and yet I feel that he helped me a lot. It was almost as if I were talking to myself, but with someone listening and trying to understand me. Blowing off steam right out loud like that and hearing it made me think about it, and helped me see things in a new light!"

That's the way a GI veteran summed up his experience with a new kind of counselor—one who helps clients solve their personal problems in their own way without giving advice. These counselors have come to believe that they have no right to direct a client's life. They feel that an individual must choose his own goals, and that if he gains enough understanding of himself and the nature of his problems he will make such choices wisely.

These counselors let the client do most of the talking. They encourage him to explore his problems and feelings freely, and come to his own solutions. They do not ask probing questions. They do not approve or disapprove of the attitudes he expresses. They do not tell him what they would do if they were in his shoes. This type of counseling focuses on the client—his feelings, his problems, his decisions. It is nondirective and client-centered.

Many other counselors, however, use a different approach. When someone comes to them for help they "take over." Like a doctor intent upon making a diagnosis, they ask many questions about the client's problems and symptoms and finally decide just what the trouble is and what caused it. Then, by means of pep talks, subtle suggestions, or logical arguments, they persuade the client to follow definite courses of action that they feel are for his own good. In the extreme of this type of counseling all the client has to do is answer the counselor's questions and follow his recommendations. This kind of counseling is directive and counselor-centered.

More and more counselors are beginning to feel that such directive techniques, in most circumstances, are not really helpful at all. Sometimes the initial problem that the client brings to the counselor is only a surface symptom of a more deeply underlying maladjustment. For example, it is well known that many students seeking vocational or educational guidance are really troubled by more basic difficulties. Yet, some counselors, because they take the initiative out of the client's hands and concentrate on the first problem presented, may miss this underlying maladjustment entirely.

Sometimes when a counselor takes the responsibility for solving a client's problems he may find the "answers," but at the expense of making the client a more dependent individual. The client may feel inferior because he had to lean on someone else for help. As a result, he may feel less confident to handle future problems on his own.

Some clients secretly resent being told what to do. Although they may listen passively enough, they will not follow the counselor's advice. Perhaps they have always

* The author is grateful to Dr. Carl R. Rogers for permission to quote brief excerpts from his writings. Examples given are reprinted by permission of the copyright owners from *Counseling with Returned Servicemen* (Rogers, Carl R., and Wallen, J. L. New York: McGraw-Hill, 1946).

rebelled against authority. Perhaps they feel their integrity is threatened when they are told what to do. Some clients, believing they have not been given the right advice, shop around until they find a counselor who tells them what they want to hear.

Sometimes clients are unable to follow the suggestions they get from counselors. Advising a depressed person to buck up or an alcoholic to control himself is like telling someone to pull himself up by his bootstraps. People may be able to force themselves into states of cheerfulness or self-control, but usually for short periods of time only. When the almost inevitable relapse sets in, they are left feeling worse than when they started. They believe they are spineless weaklings without will power.

The unsuccessful experiences of many psychologists and psychiatrists with this latter type of counseling has led to the evolution of the nondirective approach. Dr. Carl R. Rogers, professor of psychology at the University of Chicago, has most clearly defined this type of counseling. In his two books, *Counseling and Psychotherapy* and *Counseling with Returned Servicemen* (the latter written in collaboration with Dr. J. L. Wallen), Dr. Rogers has given clear expression to his belief that many individuals can work out their own problems better than anyone else could do it for them—provided they get a chance to understand themselves better.

In a majority of cases, nondirective counseling goes through three main phases: in the first, the client freely talks out his problems; in the second, he gains a better understanding of himself; and, in the third, he decides what definite actions to take. These phases don't always come in one-two-three order; they often overlap. But they have been so frequently noted that their appearance is one of the most predictable characteristics of nondirective counseling.

In the first phase, the client is encouraged to talk freely about his problems and his feelings about them. The counselor encourages him by means of attitudes and techniques which, although seemingly simple, are extremely difficult to acquire. He is friendly and receptive. He neither feels nor shows approval or disapproval. At times he says nothing more than "M-hm," "Yes," or "I see"—remarks that indicate simple acceptance. Frequently, he will restate what the client has said in such a way as to reflect and clarify the feelings expressed. Sometimes the feelings are those of despair, or hostility, or guilt—but whatever they are, the counselor, by recognizing them and reflecting them back to the client, creates an atmosphere in which the client recognizes he is being understood and accepted for what he is. Several examples might be given. The first one is taken from a case of an aviation cadet who was failing in his solo flights:

Cadet: I should have soloed long ago. And here is something. Before I joined the Navy I was an overhead electrical crane operator, and that takes depth perception, coordination, and alertness; and I'm positive that I can apply that to my flying.

C: You feel that your training as a crane operator should help you in your flying.

Cadet: That's right. And here's something else . . .

The second example is from an interview with a serviceman who was AWOL:

Dan: Well, it's a long story. I was drafted and I didn't want to go, but I went. I was sent to "X" Field for my basic training. I didn't like that at all. It was very dull, and there was nothing to do in town, and I found that all the interests I had at college were slipping away.

C: The whole business of getting into the army was pretty distasteful to you.

The third excerpt is from an interview with a bride of twenty who came to the counselor because she could not work in her disturbed emotional state:

Mrs. B: It's me and my husband. He's a sailor. We don't get along. I only knew him a week before I married him. Then he was overseas for a year, but now he's been home on a 30-day leave. It just didn't work out.

C: I see.

Mrs. B: He still loves me, but I don't love him. I don't think I did when I married him.

C: You doubt that you've ever had much feeling for him.

Mrs. B: He's nice in some ways. We talk together for hours about music and things. We have quite a little in common, but I just feel I was too

young to get married. I'd be missing something if I stayed married. I want to go around with other fellows. I'm not ready to settle down!

C: You don't want to be tied down to one man—at least not yet.

The more accepting and permissive a counselor is, the freer the client becomes to dig deeper and deeper into his feelings. He begins to see that any of his attitudes, both negative and positive, can be brought out into the open. He sees that he needn't worry that the counselor will criticize him in any way. He finds that he can drop his defensive front. He becomes free to look at himself and his problems objectively without having to protect or justify himself.

Thus, the client enters a second phase in which he begins to understand things about himself that he didn't realize before. Things that were once unrelated now seem to tie up. He sees his problems in a new light. For example, in the case of the aviation cadet, counseling uncovered an intense hatred for his unreasonably strict father. Gradually the cadet began to see the connection between his attitude toward his father and his resistance toward his flight instructor's directions:

Cadet: You know after the last interview I wondered what made me tell you the things I did. Could it be possible that the instructor is a symbol of my father? Is that hatred coming back to blot my memory? Could that possibly be significant?

C: You wonder if perhaps the instructor might be a symbol of your father?

Cadet: Yes, he was telling me what to do just like Dad always did. I fully intended to carry out the instructor's directions; I couldn't not want to do them. Maybe I forgot because I thought of Dad and wanted to forget.

Mrs. S, a young, highly educated mother who was having trouble with her boy, began to face the underlying rejection of her child:

Mrs. S: I'm afraid I'd have to say this of myself, I really didn't want Buddy. We were married two years, and I had a job. My husband didn't want me to work. We thought children would be the best solution. We felt social pressure too. With the birth rate up in the lower groups, college graduates should have children. In a limited way we were emotionally interested in him, but not deeply. And I've never adjusted to having him! It's terrible to say this!

One client saw the relation between himself and his brother in a new light:

Client: I see now why I resented my younger brother so much—why we have never been able to get along together. It was because I was envious of the attention my mother showered on him. It wasn't because of anything he did. And all this time I thought it was because he was so unfair and mean.

In the second interview the young bride talked about her unhappy home life, how she was "farmed out" to relatives, how she gradually developed the feeling that you "grabbed onto everything you could get, and hung onto it." She realized that this was the reason why she could not be generous to others and sacrifice anything for the sake of her marriage. As the interview drew to a close she said, "Maybe it would help if I just grew up."

In the third phase of counseling it is fascinating to watch the client begin to make positive decisions and to act consistently with his insights. These decisions are likely to stick because they come spontaneously from within. They grow out of the inner needs of the client himself, not in response to an outsider's advice or persuasions. For example, the soldier who had gone AWOL and talked out his situation with a counselor concludes, "I realize I've got to go back and face the music." He adds realistically, "There is no use in pretending that I won't be afraid when I go back."

Another soldier, after realizing that he didn't go to parties and dances because he feared doing the wrong thing and being rejected by others, decided to develop social skills such as dancing in order to fit in better with other people.

The young bride began her last interview by saying that she had tried being generous and thoughtful toward two of her friends and found to her surprise that it worked. They did not take advantage of her generosity, and she felt much better within herself. She spoke of her marriage in a new way: "I've decided I'm going to give marriage a try. I've never really done that. We may not be able to make a go of it, but I'm certainly not going to give him up and

give up our marriage without really working at it."

As the client takes hold of his own life and feels the original strains reduced, he takes the initiative to break off the counseling contacts. He feels as if he has matured during these counseling sessions. He feels that he has not only solved his original problems, but that he is much better prepared to face whatever future difficulties may arise.

THE nondirective approach relies heavily on the individual's own drive to achieve better adjustment. It permits the individual to choose his own way of life. It does not set the counselor up as superior to the client. It does not attempt to impose any set of values or beliefs on the client. It truly respects the individual's right to decide his own problems for himself.

A growing number of professional workers have become interested in the nondirective approach. They have found it effective with the majority of servicemen, students, children and parents, and other adults with adjustment problems. It also seems to have possibilities for the extremely maladjusted or abnormal, but experience in these extreme fields is limited and no definite conclusions can as yet be drawn.

In addition to defining nondirective techniques, Dr. Rogers has gained widespread recognition for taking the counseling process out of the mysterious realm of art and studying it as scientifically as any other human behavior. By means of phonographic recordings of counseling sessions, he and his students have been able to make objective analyses of just what goes on between the counselor and the client. Also, by means of objective psychological tests, they have successfully measured the changes in personality brought about by counseling. These accomplishments make possible scientific comparison of the different types of counseling approaches—an achievement of the greatest significance for applied psychology.

Dr. Rogers has used phonographic recordings not only in research, but also as an aid in training future counselors. The counselors-in-training, by playing back records of their sessions, analyze their mistakes under the supervision of experienced counselors.

Nondirective principles may apply to fields other than individual counseling. A beginning has been made in the use of these principles in group therapy. Nondirective interviewing in opinion polls promises to eliminate much of the factor of bias. The implications for education, industry, and social relations challenge the imagination.

Dr. Rogers himself emphasized the potentialities of his approach when he stated:

> What we now know or think we know about a client-centered approach is only a beginning, only the opening of a door beyond which we are beginning to see some very challenging roads, some fields rich with opportunity. It is the facts of our clinical and research experience which keep pointing forward into new and exciting possibilities. Yet whatever the future may hold, it appears already clear that we are dealing with an orderly and predictable process which may prove as significant a basic fact in social science as some of the laws and predictable processes in the physical sciences.

PATENTS AND UNIVERSITY RESEARCH*

ARCHIE M. PALMER

Dr. Palmer (D.C.L., University of the South, 1941) has had a distinguished career in the field of education. He has held numerous posts: acting dean of the College of Arts and Sciences of Cornell, assistant director of the Institute of International Education, associate secretary of the Association of American Colleges, and president of the University of Chattanooga. Dr. Palmer is now director of the Patent Policy Survey, National Research Council.

PATENTS are usually fortuitous by-products of research. They are not necessarily the conscious or inevitable end results of scientific investigation. This is particularly true of the products of research on the university campus, conducted with a view to expanding the frontiers of knowledge, encouraging and stimulating the spirit of inquiry, and contributing toward the training of scientific and technological personnel.

Concerned primarily with the discovery of new ideas and the understanding of nature and its laws, most scientists working in university laboratories are content to pursue their investigations without much thought of the practical application of the results. The discovery and development of patentable inventions are not conscious objects of their research efforts. They feel with Sir Henry Dale that "the primary and special function of research in the universities is to build the main fabric of knowledge by free and untrammelled inquiry and to be concerned with the practical uses of it only as these arise in the course of a natural development."[1]

However, many new ideas, discoveries, and inventions, the result of experiments undertaken with quite a different purpose in view, may have valuable commercial application or require protection and control in the public interest. They may not only be essential to scientific and technological progress and to cultural and social advancement, but these new ideas may also be basic to industrial development and expansion. The protection and control provided under the patent laws may have to be invoked to obtain the greatest public benefit and usefulness from these products of scientific research.

Some scientists in university circles take the attitude that the publication of the results of scientific research and their dedication to the public are sufficient. But, as President Karl T. Compton, of the Massachusetts Institute of Technology, said in his annual report for 1932:

> Responsibility does not always end with the mere publication of a patentable scientific discovery or invention; the public benefits derivable from the patent laws and contemplated by the framers of those laws should not be lost through a failure to solicit patent protection.[2]

Discoveries or inventions possessing commercial application that are merely published and are thus made available to everybody equally are seldom adopted. As Elihu Thomson so aptly put it:

> Publish an invention freely, and it will almost surely die from lack of interest in its development. It will not be developed and the world will not be benefited. Patent it, and if valuable, it will be taken up and developed into a business.[3]

Interest in science and scientific research, particularly in the natural sciences and in their application to engineering and medicine, was intensified and accelerated by our experiences during the war. Research workers and scientific investigators from the university campus and the industrial laboratory aided materially in the magnificent record our nation made in war production and military achievement. Returned now to the campus and the laboratory, on release from wartime responsibilities and occupations, they are more research-minded than ever. Interest in research is being further stimulated by government and industry, which are turning to

* From an address before the Association of American Universities, Iowa City, Iowa, October 24, 1947.

universities and technological institutes for assistance in solving postwar problems.

The situation is made more acute by the critical shortage of scientific and technical personnel and the need for developing a new crop, grounded in fundamentals and trained in research procedures. This new supply of qualified scientists to meet the needs of the future as well as the immediate present must come from the universities. At the same time universities are expected to continue as centers of basic research.

American science faces a challenging future. Can science be mobilized for peacetime purposes as effectively as for war? Will scientific investigation be conducted under conditions favorable to the search for new knowledge? Can we build upon and utilize our wartime experiences and the present research consciousness among scientists and the public generally? How will our universities, the primary source of independent scientific investigation, respond to the challenge?

These are questions of paramount importance if this nation is to discharge its responsibilities and assume leadership for peace and progress in the postwar era. Social and national security, public and private health, and economic prosperity and well-being depend today, as never before, upon the rapid extension of scientific knowledge and the effective application of that knowledge.

Of direct concern to university administrators and scientists engaged in the formulation and conduct of research programs is the policy or procedure to be followed in handling the results of scientific investigation. How can the greatest public benefit be obtained from new discoveries and inventions? Specifically, how should these discoveries and inventions be administered in the public interest, taking into account the objectives of the institutions and the over-all welfare of the scientific workers?

Whether we are to enter upon a rich era of productive research, profiting from our wartime experiences and capitalizing on the present research consciousness among scientists and the public generally, will depend to a large extent upon the philosophy behind our university research programs and the administration of those programs. The public welfare, educational objectives, direction of scientific thought, and the advancement of knowledge are all involved.

As a service to American higher education and to the scientific fraternity, the National Research Council has been making a comprehensive study of this problem. As a first step, a factual survey[4] is being made of the prevailing policies, procedures, and practices in educational institutions and nonprofit organizations for the administration of patentable results of scientific research, with a view to the early publication of the findings for the information and guidance of all concerned. Through correspondence, conversations, and visits to university and other research centers, information has been assembled concerning existing practices and present thinking, in administrative and scientific circles, about research policies and patent management programs.

At present there is a wide diversity of practice among educational institutions—and even at the same institution—in dealing with patentable discoveries and inventions growing out of scientific research. There is no common pattern of policy statement, administrative procedure, recognition of the inventor, determination of equities, assignment requirement, patent management plan, distribution of proceeds, or protection of the public interest. Nor is there any convenient grouping according to type or size of institution, complexity of university organization, or kinds of research undertaken. Existing practices vary from strictly drawn patent policies to laissez-faire attitudes and even an unwillingness to become concerned with patents.

Some institutions follow a hands-off policy, leaving to the individual inventor the responsibility for determining what disposition is to be made of the product of his research efforts. Others take the position that the institution has an interest in all research activity on the campus and have established formal patent policies or follow generally accepted practices for handling any patentable discoveries that may result. Still others observe a definite policy of not having a patent

policy. A great many, however, have given little or no consideration to the patent problem, despite the increasing volume of scientific investigation on the campus.

Through the years certain institutions, faced with immediate situations, have formulated more or less definitive patent policies. Yet, less than forty such policies have been formally adopted thus far, more than half of them during the past five years. At a number of other institutions practices and procedures are being currently followed which, though not definitely formalized, are generally accepted as applicable to research activities throughout the institution. A few have adopted special policies or recognize general practices for dealing with those results of scientific investigation that affect public or private health. Others have developed policies and practices only with respect to sponsored research.

Many of the formalized policies and most of the prevailing practices are currently under review to meet changing postwar conditions and current considerations in the institutions. The need at this time for critical examination of the whole question of what to do with the patentable products of research is well recognized. At a number of institutions, many of which have not previously had any patent policy, faculty and trustee committees are currently studying the question, with a view to formulating new, or revising existing, policies.

At many institutions each case is decided on its merits in accordance with a general policy or, in the absence of such a policy, by agreement among the parties concerned. A few still feel that they discharge their responsibility by merely publishing the results of investigations or by securing patents and dedicating them to the public. Others accept full responsibility for obtaining patents and administering the patent rights in the public interest. Many exercise control over the patents by issuing licenses and accepting royalty payments, either directly or through agents whom they have designated to manage their patents.

Some recognize the rights and interests of the inventor and share the proceeds with him, either under a prior contractual arrangement or by mutual agreement, but there is no uniformity in the division of the financial return from patents between the inventor and the institution. Even in those instances where the proportion to be given the inventor is specified in accordance with a general policy, there is a wide variation among institutions in the amount allotted to the inventor. In other institutions the inventor's share is determined in each case after consideration by a special faculty or administrative committee. A few institutions include patent provisions in their contracts of employment, in some instances for all faculty members, but more often limited to members of the staff whose entire or major responsibility is research, especially contractual research.

At most institutions the compulsory assignment of patent rights is not considered desirable, except when necessary in connection with cooperative or sponsored research. Voluntary assignment is preferred and in many institutions is encouraged and facilitated through procedures and special machinery for handling patents set up within the institution. In many instances the services of an outside organization closely related to the institution and under agreement to act as its patent management agent are employed.

Some institutions administer patent applications, and the resulting patents, directly, utilizing their regular administrative personnel, special units within the institutions, or separately organized agencies directly responsible to the boards of trustees of the institutions. Others, for legal or fiscal reasons, use the facilities of separately incorporated patent management foundations, independent of, but closely allied to, the institutions. Still others have entered into agreements with Research Corporation, a nonprofit patent management foundation, to handle patentable discoveries in their behalf, with full protection of their interests and those of the inventors and the public. Most institutions endeavor to avoid becoming involved in the intricate legal and commercial aspects of patent management, mainly because they lack personnel with the requisite specialized knowledge and experience.

In order to provide incentive and encourage personal research interests of faculty members, most educational institutions place little or no restriction on the disposition of inventions and patentable discoveries resulting from scientific research conducted on an individual's own time and at his own expense, even though the institution's facilities and equipment may have been used. Such inventions are considered to be the exclusive property of the inventor, and he retains the full patent rights and complete freedom to dispose of them as he deems proper.

Institutions with formalized patent policies usually recognize, by explicit reference or by implication in formal policy statements, that an invention or discovery not related to the individual's regular teaching or research responsibilities belongs to the inventor, and accordingly waive all claim to a share in possible financial returns. Similarly, at many of the institutions which, in the absence of an established policy, follow generally accepted practices—as well as those which observe a laissez-faire, or hands-off, policy—ownership of patents resulting from personal research rests with the inventor.

In the absence of established policies, some institutions consider each case on its merits, leaving it to the judgment of the faculty member whether he should bring what may seem to be patentable discoveries to the attention of the president or designated administrative officer or faculty committee charged with consideration of research and patent problems. A few universities with definite patent policies require that all potentially patentable discoveries, as well as the intention to apply for patents, be brought to the attention of the administration, either directly or through appropriate committees.

A number of institutions have special committees or boards to which are referred patentable discoveries, questions of the institution's interest in them, and the desirability of securing patents at the institution's expense. When recommending the specific action to be taken in each case, these committees usually also determine what recognition or reward, if any, should be given to the inventor. In many instances the inventor is required or advised to assign his rights to a patent management organization designated by the institution to represent its interest and handle the commercialization and general administration of the patent rights.

In the administration of formal patent policies, a number of institutions use these committees or the patent management agencies to advise and aid faculty members on matters of patentability, prosecution of the patent application, commercialization of the patent when issued, and general business aspects of patent management. Through these committees and the regular university administrative channels, and also through the facilities of patent management foundations, means are provided whereby, by either voluntary or required assignment of their patent rights, faculty members may be relieved of the burdensome legal and administrative problems associated with the commercial exploitation of patents.

Frequently these committees also have responsibility for determining whether the institution has any interest or equity in the discovery and for defining what action should be taken in line with the prevailing patent policy or accepted practice of the institution. In many instances it is difficult to determine the extent to which incidental or permitted use of the equipment and other facilities, membership in the company of scholars assembled on the campus, professional contacts with colleagues and others connected with the institution, and the general intellectual atmosphere and surroundings contribute to the evolution of patentable ideas. Certain institutions require reimbursement of whatever contribution in institutional time, money, or facilities has been made to the production of a patentable discovery, even though the patent rights remain the sole property of the inventor.

Even where inventions and other developments grow out of research entirely or substantially financed by the institution, there is considerable variation in the patent policy observed, the procedures followed, and the recognition of the inventor. However, when the research is part of the regular duties and responsibilities of a faculty or staff member,

it is generally the practice to require assignment of title to such inventions and developments, as well as any patent rights that may accrue from them, to the institution or to its designated agent. In such cases, the institution bears the costs of obtaining the patent and assumes responsibility for its exploitation. Provision is usually made for the patent rights to revert to the inventor if the institution or its designated agent does not file a patent claim within a reasonable time, which is sometimes but not always specified in the assignment agreement.

Most institutions require full-time research personnel and others employed on special research projects to sign patent assignment agreements covering all patentable ideas and discoveries that may result from their investigations. Such agreements are generally required of full-time research employees in state agricultural and engineering experiment stations, and also of those employed on projects conducted in, or under special research institutes affiliated with, educational institutions.

Few patent policies include any reference to patentable discoveries resulting from student research, except where the student is employed or receives specific fellowship aid under an industrial research contract. In general, inventions made by students, including those on academic scholarships and fellowships, are considered to be the private property of the students. Nevertheless, the question of requiring students to sign patent waiver agreements is occasionally raised, especially when the students are given scholarship aid. The acceptance of such scholarship aid, however, is usually not considered as changing the status of the student in regard to title to inventions or developments, since such scholarship funds are in general administered, rather than controlled, by the institutions. The rights of the student include the right to assign or otherwise dispose of his patent rights.

An important—and controversial—aspect of the patent problem is concerned with patentable products of scientific research that affect public and individual health, particularly discoveries and inventions of a medical, pharmaceutical, therapeutic, or hygienic nature. Those universities that have comprehensive patent policies usually include such discoveries, processes, developments, and inventions within the scope of general over-all policies. A few provide specifically for a different treatment of medical discoveries, designed to discourage patenting except when it is considered necessary in the public interest and then without consideration of profit, either to the individual or to the institution. A considerable number have no fixed policy: when cases arise, each one is handled individually, usually without following any uniform pattern except, as a general rule, to discourage investigators from seeking patents on such discoveries.

The prevailing practices of educational institutions, especially those with medical faculties, are influenced to a considerable extent by the traditional attitude of the medical profession as to the ethics of patenting medicinals and medical appliances. Such an attitude, however, does not necessarily preclude patenting a new process or discovery in the public interest. Yet, many scientists working in this field take the position that the results of their research, both patentable and otherwise, should be shared "without fee or stipulation." In so doing they often fail to differentiate between patenting for personal gain and patenting in the public interest.

Through the centuries, medicine has given freely of its discoveries for the benefit of mankind, and these discoveries have become the property of all who cared to employ them in the control of disease. However, as medical research has become more complex, involving specialized investigations in the fields of biochemistry, physiology, physics, and associated branches, great numbers of full-time research scientists in the hospital and the laboratory work with members of the medical profession but are not bound by the same ethical principles. Many important medical preparations and techniques have been developed in university laboratories, often at considerable expense to the institutions.

The patent problem is not a settled one in the medical schools, and a wide difference of

opinion exists among their faculty members as to the ethics of patenting a medical discovery, but in many of these schools the question is being given thoughtful consideration. Much of the stimulation for the establishment of definitive patent policies stems from problems growing out of research projects sponsored by outside agencies, especially commercial firms. Frequently, such practices as are currently followed are concerned solely or mainly with the results of scientific research conducted under grants from those outside sponsors.

Scientific research sponsored and supported by industry is a major activity on many a university campus. It is conducted both as an integral part of the educational program and as a special service to industry. Support comes in many forms: as unrestricted gifts, grants-in-aid, industrial fellowships, and the financing of specific research projects.

Industry support of university research is not a new phenomenon. For years industrial corporations and trade associations, as well as individual industrialists, have provided funds for the conduct of both fundamental research and specialized developmental, or applied research, investigations. Progressive business executives recognize the potential value of the research facilities and the scientific personnel in universities and technological institutions in the promotion and expansion of industrial progress.

In its return to postwar status, industry has been turning to the colleges and universities for assistance in solving its reconversion problems. Unable to provide within their own organizations means for producing new ideas for the improvement and replacement of obsolete facilities and processes to meet postwar conditions, large and small businesses alike, as well as trade associations and groups of related industrial firms, have been seeking the services of educational institutions in research on specific developmental problems.

Despite the heavy teaching load resulting from swollen postwar enrollment and their own lack of adequate instructional personnel, educational institutions have been quick to respond to this new call upon them. A number have for years been rendering such service to industry, both on an institutional basis and through consulting and research work on the part of individual staff members. This has been particularly true in state universities, land-grant colleges, and technological institutes. However, largely as the result of experiences with war contracts and observation of what others have done and are doing, there has been a material increase during the past several years in the number of colleges and universities offering research services to industry.

An appendix in the National Research Council's recently published directory of industrial research laboratories[5] lists approximately three hundred educational institutions that offer such services, and the list is admittedly incomplete. At a number of institutions, special research institutes, corporations, and foundations, usually independently incorporated but closely related to the institutions, have been established for the conduct and administration of sponsored research programs, as well as the management of the patentable results of the research.

Encouraged by the success, often more apparent than real, of certain of these organizations, more than seventy colleges, universities, and technological institutes have set up such agencies, many within the past three years, and others are contemplating similar action. These organizations are located in all parts of the country and at all types of institutions, large and small, public and private—at endowed universities, state universities, land-grant colleges, technological institutes, and small colleges alike.

Some are integral parts of the administrative and organic structure of the institutions concerned, operating as special departments or divisions. Others are independent nonprofit foundations, separately incorporated but closely affiliated with the educational institutions and utilizing their regular personnel and facilities. A few maintain special research laboratories and separate personnel distinct from the regular teaching

staffs of the institutions. Combinations of full-time services of special research workers and part-time research and supervisory services of regular teaching members are found at a number of institutions.

Many of these agencies have been set up to provide convenient means for relieving the institution's regular business and administrative staff of contractual relations with research sponsors and of patent management problems. In some instances they are also concerned with the general development of new sources of financial support for the institution itself. Still others are designed to provide machinery for conducting sponsored research activities and for handling patents, particularly where restrictive statutory provisions make it either impossible or undesirable for the institutions to perform these services themselves.

The creation of these special research organizations and the conduct on the university campus of extensive research programs supported by industry, and also by government, raise problems whose implications are more far-reaching than is immediately apparent. What influence will such programs have on the character of scientific investigation in our American colleges and universities, and what effect will they have on the educational programs of those institutions? Will the emphasis be on developmental research? Will basic research suffer? Will there be greater interest in immediately usable end results than in the search for new knowledge? Will too much reliance be placed upon the financial return from patents and from sponsored research projects in balancing the institution's budget? Will due consideration be given to the uncertainty of that revenue and its possible effect upon other sources of income and upon the tax-free status of the institution?

There is a wide diversity in the organization and operation of these agencies and in their handling of the policies and responsibilities of sponsored research programs. There is no uniformity in the terms or conditions under which sponsored research projects are accepted or conducted, nor in the determination of the charges made. Some institutions have established specific policies for handling such research; others make the best arrangements obtainable in each case. Some will accept only projects which are definitely related to their educational programs and which can be performed by faculty members and students as part of their regular activities. Others have set up special facilities for sponsored research, employing personnel who devote full time to such activities. A number have established special bureaus or divisions within the institution to relieve the faculty and regular administrative personnel from contractual relations with research sponsors.

The patentable products of such research are handled in many different ways, the ownership and control of patent rights sometimes being retained by the university but more often being turned over to the sponsor under a predetermined contractual arrangement. Certain institutions are unwilling, and a few refuse, to undertake research projects likely to entail patentable developments. Others are willing to undertake such research projects only when they retain complete control over both the patent rights and the publication of the results of the investigation. Still others will enter into contracts under which the sponsor receives, for a consideration, ownership of all patentable discoveries, as well as full and confidential report on the research findings.

It is usual practice for educational institutions to retain control over the publication of the results of research conducted on the campus. When an investigation is financed through outside funds, that control is frequently but not always exercised subject to prior consent of the sponsor, and publication is withheld for a reasonable time to protect patent applications and the interests of the sponsor in commercial development of new discoveries or processes. A few institutions turn over all the results to the sponsor, including publication privileges as well as patent rights, merely reserving approval of any reference to the institution or its part in the investigation. In most instances they

proscribe use by the sponsor of the name of the institution in any way.

PATENT management is a complicated business and is expensive. It requires a high degree of legal competence, administrative astuteness, and promotional zeal—a combination of talent not always readily available in an educational institution. The patent search is a specialized technical service. The preparation and processing of patent applications are exacting work for legal counsel. The administration of patent rights demands careful attention to intricate details and constant watch for infringement and interference. The exploitation and disposal of patents, through sale and licensing agreements, require salesmanship of a high order.

It is natural, therefore, that most educational institutions make every effort to avoid becoming directly involved in the intricate legal and commercial aspects of patent management. Some endeavor to accomplish this by adopting a hands-off policy and refusing to handle the patents. Others have established, or have encouraged the organization of, separately incorporated patent management foundations, independent of, but closely related to, the institution by the terms of their charters and by the membership of trustees, administrative officers, and faculty on the foundations' boards of directors. Still others have entered into agreements with Research Corporation to act as their patent management agent.

A few attempt to handle patents as a part of the routine duties of already established administrative units, such as the comptroller's or business office, or through specially designated committees responsible directly to the administration or to the trustees of the university. A number have faculty committees on patents, which exist primarily for the purpose of ensuring that pertinent institutional regulations are observed. Often these committees serve as advisory bodies and are charged with recommending action on matters that range from the desirability of taking out a patent to the determination of equities.

There are at least three distinct equities or interests involved in patentable discoveries or inventions resulting from scientific research in an educational institution: the inventor or inventors, the institution, and the general public; to which must be added a fourth, the sponsor or supporter of the research, in the case of sponsored or cooperative research. When further developmental work is necessary, a fifth interest may be involved, although frequently it is the same as the sponsor or supporter of the original research.

The recognition and protection of these several and diverse interests naturally complicate any individual situation. Self-interest, personal rights, institutional policies, employer-employee relations, academic freedom, contractual relations, patent law, business practices, commercial competition, and the variables in individual cases are some of the elements that contribute to the problem. Nevertheless, to be equitable and effective a patent policy must provide for such recognition and protection, placing the responsibility where it can be best discharged, most expeditiously and with the minimum of burden on the regular administrative and teaching staffs of the educational institution.

REFERENCES

1. DALE, H. Academic and Industrial Research in the Field of Therapeutics. *Science*, 1933, **77**, 525.
2. COMPTON, K. T. Quotation from annual report, M.I.T. *Tech. Rev.*, 1932, Dec., 101.
3. THOMSON, E. Quotation from address before graduating students, M.I.T. *Elec. World*, 1920, **75**, 1505.
4. PALMER, A. M. University Patent Policies. *Ass. Amer. Col. Bull.*, 1947, **33**, 167–174.
5. *Industrial Research Laboratories in the United States* (8th ed.). Washington, D. C.: Nat. Res. Council, 1946. P. 349.

SCIENCE ON THE MARCH

PLANT PROTECTION MUTATES

President Truman has called upon Americans to furnish hungry Europe with 200,000,000 bushels of grain, chiefly through eating and wasting less. L. S. Hitchner, representing the insecticide-fungicide industry, has wired the President that this much surplus grain can be made available without any reduction in home consumption, by full-scale utilization of old and new knowledge of the prevention of crop losses caused by diseases, insects, rats, weeds, and other pests.

World War II was a mighty stimulus to the development of new and more potent chemical pesticides. There was an insufficient supply of the old-fashioned chemicals of protection such as copper, mercury, and arsenic; military demands for protection of personnel and matériel in pest-ridden climates competed for these scarce chemicals with civilian demands for greater crop production through pest control.

The fundamental research essential to the development of new chemical pesticides had been quietly advancing in the prewar years. Now came the pay-off, with such developments from the basic research as the revolutionary insecticide DDT, the weed killer 2,4-D, the powerful rat poison ANTU, and a host of other new, potent pesticides.

Less widely publicized, but no less revolutionary, have been the recent developments in fungicides for protecting crops from disease and for proofing fabrics and other organic materials from fungus destruction. More progress on fungicides has been made in the past decade than in the preceding half century. It is the knowledge of the effectiveness of this arsenal of new pesticides that gives agricultural leaders the assurance that Europe's need for foodstuffs can be met and exceeded by pest-control alone.

The basic research that has made possible the development of radical departures in fungicides has centered around the bioassay of chemicals to determine their fungicidal potentialities. Chemical assay is not sufficient; the toxicity of a given chemical to fungi, living organisms, is conditioned by a variety of factors the effects of which can only be determined in direct tests of chemical against fungus.

In the bioassay of fungicides ingenious equipment is used, in which possibly useful materials are sprayed at a glass target under precisely controlled conditions. Spores of a standard strain of fungus are then tested for germination on the sprayed glass, also under controlled conditions, and this work is supplemented by tests in which living leaves replace the glass target. Using such techniques, thousands of possible fungicides can be "screened," and the few promising ones may then be tested for value in controlling fungi under greenhouse and field conditions.

The bioassay of fungicides is not merely a culling out of hundreds of unsatisfactory compounds, selected at random, in favor of a few promising ones. With suitable assembly and analysis of data from the bioassay, it is possible not only to determine that a given chemical is toxic to fungi, but to shed light on the nature of the fungicidal action, the type and purity of the compound, and the nature of interaction of two chemicals in a spray mixture; the bioassay is the key to the dynamics of fungicidal action. Thanks to it, plant pathologists no longer need to test chemicals at random on the chance that one might be useful; it can now be predicted that compounds having certain molecular configurations will be fungicidal.

When the dosage of the chemical is varied, there is corresponding variation in response of the fungus spores. If dosage is plotted against toxic effect on a probability-logarithmic grid, there results the "dosage-response curve," a beautiful tool revealing much about the dynamics of fungicidal action.

If the dosage-response curves of two chemicals intersect *X*-fashion, one compound is

more toxic than the second at higher dosages, and the reverse is true at low dosages. If the two intersect *V*-fashion, one chemical loses its toxicity with dilution more rapidly than the other. If the dosage-response curve has two peaks, separated by a valley, this means that with high dilution a fundamental change in the nature of fungicidal action occurs, such as a change from molecular toxicity to ionic toxicity. Comparing the dosage response curves of two chemicals, individually, with the curves of their mixture reveals whether the chemicals in combination reinforce each other (synergistic effect) or antagonize each other. Moreover, the curves in this case give some clue to the nature of interaction of spray ingredients, whether it is simply additive or subtractive, or whether it is "potentiated," altering the mechanism of toxicity. For an extended account of the bioassay, the dosage-response curve, and the methods of action of fungicides, the reader is referred to the recent excellent book by J. G. Horsfall: *Fungicides and their Action* (Chronica Botanica Co., 1945).

For more than half a century, chemical control of plant diseases had been based almost entirely on copper, the toxic ingredient of Bordeaux mixture, and sulfur, alone or with lime. Today, thanks to the basic research centered about the bioassay, these veterans of chemical crop protection are rapidly being forced into the background by new and better organic fungicides. In the 1930s were introduced the organomercury compounds, which for a time monopolized the practice of seed treatment, a practice so successful that it has been compared with hybrid corn as one of the most outstanding agricultural developments of recent years.

In rapid succession during the last decade there have appeared other spectacular agents of crop protection, among them the carbonyl compounds such as Spergon (tetrachloro-*p*-benzoquinone) and Phygon (2, 3-dichloro-1, 4-naphthoquinone), as seed protectants and sprays lacking the poisonous qualities of mercury compounds. Then there are the triocarbamates, sulfur-containing organic compounds as outstanding in the field of plant pathology as their relatives, the sulfa drugs, in medicine, including the valuable tetramethylthiuram disulfide (Arasan or Tersan) for seed treatment and sprays, and such remarkable spray materials as ferric dimethyl dithiocarbamate (Fermate, etc.), zinc dimethyl dithiocarbamate (Zerlate, etc.), and disodium ethylene bisdithiocarbamate (Dithane), which, against many important crop diseases, have already largely supplanted copper and elemental sulfur. Even more remarkable, in some respects, are the new quaternary ammonium fungicides, compounds with 5-valent nitrogen, the most outstanding of which is phenylmercuritriethanol ammonium lactate (Puratized N5), a compound of a double fungicidal character by virtue both of its quaternary ammonium structure and its mercury content, with toxicity to fungi at the extremely high dilution of 1–5 parts per million.

The new organic fungicides are not merely toxic to fungi; many other useful properties have been considered in their development, such as their nontoxicity to plants and humans, their compatibility with other spray ingredients, and their tenacity of adherence to foliage despite heavy rains.

Until recently fungicidal sprays or dusts have been regarded as *protectants*—preventing the infection of healthy tissues but of no use once the tissues have become infected. This classic conception has been upset both by study of the action of new fungicides and reinvestigation of older ones. It has been found that such products as Puratized, sodium dinitro-*o*-cresoxide (Elgetol, Sinox), and even pure copper sulfate are *eradicant*, not only protecting plants but actually inhibiting or destroying established infections.

Parallel with the development of these new seed protectants, sprays, and dusts, has been the appearance of other chemicals of crop protection designed to rid soil of undesirable inhabitants, fungi, nematodes, and weed seed. Already these are being produced at such reasonable cost that they are being used on increasingly large acreages, and this is often followed by extraordinary increases in crop yields.

These soil disinfestants are volatile organic compounds, and their action is essentially a fumigation of the soil. Fumigation, although widely used in insect control, has had

little place in protection against plant diseases, but recently it has been found that the "blue mold" diseases of tobacco and cabbage seedlings can be effectively controlled by fumigating the seedbeds with benzene or paradichlorobenzene.

Outstanding as are these developments, they still do not complete the tale of new discoveries in the conquest of plant disease. Plant pathologists have always concentrated their attention on prophylactic measures, aimed at protecting plants, with little interest in trying to cure them once they are diseased—quite the opposite of traditional human medicine. Now a chapter on the therapy of plants is beginning to unfold, with particularly interesting findings on the chemical treatment of trees suffering from wilt, root rot, and virus diseases. In some cases the effect is not directed against the agent of disease but is a neutralization, or "antidoting," of its toxic products. A diversity of chemicals is being used in such studies, even including the new antibiotic drugs, such as penicillin.

Still another chapter might be written on revolutionary new methods of application of pesticides. The crop-protection airplane is becoming commonplace, and ground equipment of radical new types is replacing the old conventional sprayers and dusters. New principles in pesticide application are involved, including the use of a high-velocity air current as a diluent for concentrated dusts and sprays, equipment that applies the chemical in an artificial dew or fog, and the aerosol method where the pesticide carrier is a highly volatile organic fluid. These new developments in pesticide application are greatly decreasing the cost of crop protection in both labor and chemical required per acre.

It is too early to have a balanced perspective of these developments of the past decade and their impact on agricultural economy, but there is little question that we are witnessing the dawn of a new era in crop protection—one that may come to be regarded as significant as the advent of mechanized agriculture.

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SEX IN BACTERIA AND VIRUSES

THE manifestations of sex are known to all, but its biological meaning is clear to few since it can only be understood in terms of evolution. Evolution by natural selection takes place because there are present in natural populations large numbers of different combinations of hereditary characters, some of which have greater chance than others of survival and propagation.

New characters arise by mutations, that is, by rare and unpredictable changes of determinants of heredity, or "genes." One mutation seldom gives rise to an individual better fit for life than the original. Favorable mutations do, however, arise, if only with low frequency. How will they be made use of for the survival and improvement of the species? As pointed out by Muller in 1932, in asexual organisms, that is, in organisms reproducing from one parent only, a mutant gene will be tested for its survival value only in combination with the other genes already present in the line of descendance in which it has arisen. If several such favorable mutations arise in different lines, they will compete with one another and tend to crowd each other out. Combinations of several favorable mutant genes can only be formed if the mutations happen to occur successively within the same line of descent, necessarily a very rare event. Much good material for evolution will thus be wasted.

Sexuality prevents or at least greatly reduces this waste. In the process of sexual, biparental reproduction the genes of one individual have at each generation a chance to be reshuffled with those of another individual, producing a variety of offspring types. In natural populations, these are generally all more or less different from one another and from their parents. Reshuffling occurs because the genes are carried in chromosomes, nucleoprotein threads present in various number of pairs in the nucleus of each cell, each

member of a pair being contributed by one parent. The variety of combinations, arising from the random assortment of the members of each chromosome pair, is further increased by exchanges of parts between chromosomes of each pair within the parents (crossing-over) in the course of the formation of the sexual cells, the sperm and the egg. As a consequence, no set of genes of a grandchild is identical with any one of those found in his four grandparents.

Moreover, the very increase in the total number of gene combinations resulting from sexual reproduction is an advantage for the species: if environmental conditions vary, gene combinations that might previously have been unfavorable may prove useful in the new exceptional situation and increase the chances of survival of the species.

Finally, mutant genes that would be a handicap in one combination might prove actually advantageous in other combinations. Sexuality, so to speak, gives them a chance. In Muller's words, "sexuality, through recombination, is a means for making the fullest use of the possibilities of gene mutations."

Since sex is such a powerful factor of survival, evolutionists have often wondered how organisms like bacteria manage to survive at all if they are, as is generally believed, completely sexless and only reproduce by repeated fissions. As pointed out before, the number of available genetic combinations will be quite limited, and the chance for the appearance of more favorable ones rather small. In part, this limitation might be compensated for by the large size of bacterial populations, which allows the occurrence of even rare mutations, so that a number of different combinations might actually be formed within one hereditary line. In the long run, however, bacteria would probably be better off if they had some kind of sex.

In 1946 Lederberg and Tatum announced the results of experiments strongly suggesting sexlike phenomena in bacteria. From one original parent strain of *Escherichia coli,* a common intestinal bacterium, capable of manufacturing all its protoplasmic materials and of growing in a mixture of very simple chemicals (sugar, ammonia, and inorganic salts), these authors first isolated several "nutritionally deficient" mutants, that is, mutants unable to manufacture some essential chemical needed for their growth. These mutants are easily obtained by irradiation of the parent strain with ultraviolet light. A mutant can only grow if the chemical or chemicals which it is unable to produce is supplied in the culture medium. Indirect evidence, mainly the analogy with the situation in the mold Neurospora, had already suggested that each nutritional deficiency is tied up with a modification of one gene.

Let us suppose that the original strain is capable of synthesizing the four substances A, B, C, and D. A mutant $A-B-C+D+$ (deficient for compounds A and B as a result of two mutations, but capable of manufacturing C and D) is grown together with another double mutant with reverse synthetic abilities, $A+B+C-D-$ in a medium containing plenty of all four substances A, B, C, and D. If the cells of the two mutants, while growing together, undergo some type of sexual fusion, exchanges of genetic determinants might occur. Among such exchanges there may be some leading to the formation of cells $A+B+C+D+$. These would be nutritionally identical with the original strain (prototrophic, from *protos,* "original," and *trophe,* "nutrition") and, like the latter, easily distinguishable because able to grow in the absence of all four substances A, B, C, and D.

Strains with at least two deficiencies had to be used to eliminate the possibility of prototrophic organisms being produced by one mutation. The chance of two mutations occurring in the same line is low enough to be completely negligible.

Lederberg and Tatum actually found prototrophic cells present in mixed cultures of a number of different mutants with several deficiencies and interpreted them as proof that genetic recombination had occurred. This conclusion was supported by the fact that, when strains differing by three or more mutations were grown together, there appeared in the mixed cultures some cells that showed all possible combinations of the characters of the two parents, indicating that the transfer could involve different numbers of genetic determinants.

The mechanism of fusion and gene ex-

change in bacteria is still unknown, and the results may need careful checking before their interpretation as genetic recombinations can be considered as proved beyond doubt. It seems likely that, if sexual phenomena like these are actually proved to occur at all in bacteria, they will be found to be frequent enough to play an important role in bacterial evolution. It would, in fact, seem strange that a mechanism so useful from the evolutionary standpoint, once arisen in a group of organisms, might remain confined to just a few strains of bacteria. The very fact of having been found in one of the few strains carefully tested would suggest its widespread occurrence.

If bacteria may have sex, what about the filterable viruses? This might seem to be asking too much indeed. After all, viruses are ultramicroscopic entities reproducing inside living cells and often thought to be hardly more than nucleoprotein molecules. And yet we know that some of them undergo changes very similar to sex in their genetic consequences. This was discovered in the course of work on bacteriophages, the viruses that reproduce inside bacteria somewhat as the virus of infantile paralysis reproduces in the cells of the spinal cord of man, or the virus of influenza in the lungs.

Bacteriophages can undergo a number of different and independent mutations, and this writer and Hershey have in recent years described a number of them. In 1946 Delbrück and Bailey infected the same bacterial cells simultaneously with two bacteriophages carrying different mutations. As a result of this mixed infection, new types of viruses, different in their combination of characters from those that had entered the cells, were produced. These workers, for example, used two bacteriophages, T2 and T4, somewhat related but serologically and otherwise distinct, which can give mutants called T2r and T4r. These are easily distinguishable from T2 and T4 by the peculiar appearance of the zones of destruction, or "plaques," that they cause in a layer of bacteria grown on solid media. When particles of both viruses T2 and T4r entered the same bacterial cells, the new phage produced inside these cells consisted of four types: T2, T2r, T4, and T4r. It was necessary to conclude that the property designated as "r" had either been induced or else transferred from one phage to the other in the course of reproduction. Clearly, this amounted to the same result as that of a sexual cross, that is, the formation of new combinations of hereditary characters. These results were confirmed and extended by Hershey with the study of several other mutant properties. They can be taken to prove that, whatever the unknown mechanism of virus growth inside the cell may be, it must allow for some kind of reshuffling between the groups of hereditary determinants brought into the same cell by different virus particles.

In addition to this, this writer has recently found that genetic recombinations between viruses can do more than alter properties of the above kinds; they can restore their reproductive ability after this has been lost. Bacteriophage particles were inactivated by ultraviolet rays and, as a result, lost their ability to be transmitted from one bacterium to another without losing that of penetrating the host. Two or more such "inactive" particles were made to enter one bacterium, and active particles of bacteriophage reappeared. We have obtained evidence suggesting that this "resuscitation" depends on the fact that particles that have been damaged by radiation in different parts can transfer portions while inside the same host cell, with an opportunity for the formation of normally active particles. In other words, it seems that a lethal mutation can occur in any one of a number of parts of a virus particle, each of these parts being replaceable by transfer from another virus particle.

It is interesting to remember that more than ten years ago Berry and Dedrick discovered that the virus causing rabbit fibroma could be transformed into the virus of myxoma, a closely related virus of rabbits, by simultaneous injection of active fibroma and inactive myxoma virus. This phenomenon may find its explanation in some transfer of genetic properties similar to that occurring in bacteriophages.

It would be rash to draw from these results any general conclusion as to the origin of sexual processes and their evolution. It is

possible that different mechanisms permitting genetic recombination have appeared independently at various stages of the evolution of the biological world and that not all such mechanisms have developed along similar lines. The frequency of occurrence of genetic recombinations is likely to be very low in the case of bacteria and viruses, where reproduction by fission is probably the rule and conjugation between different individuals resulting in gene recombination, when present, is the exceptional occurrence. It is, therefore, possible that mechanisms for genetic recombination that could prove too radical for higher organisms, where they would be repeated at each generation, might work successfully in viruses or bacteria. It would clearly be dangerous to try forcing analogies from the genetics of plants and animals to that of bacteria and viruses until the latter rests upon a more solid factual basis.

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LIST OF AFFILIATED SOCIETIES AND REPORTERS APPOINTED BY THEM TO CONTRIBUTE TO "SCIENCE ON THE MARCH"*

Section B (Physics)

American Meteorological Society	Wallace E. Howell
Optical Society of America	Frank Benford

Section C (Chemistry)

American Chemical Society	Walter J. Murphy

Section D (Astronomy)

American Astronomical Society	Joseph Ashbrook
The Meteoritical Society	John A. Russell

Section E (Geology and Geography)

American Geographical Society	John K. Wright Raye R. Platt
Seismological Society of America	C. F. Richter
The American Association of Petroleum Geologists	M. G. Cheney R. K. DeFord
The Paleontological Society	Harold E. Vokes

Section F (Zoological Sciences)

American Society of Mammalogists	E. R. Hall

Sections F and G (Zoological and Botanical Sciences)

Genetics Society of America	S. E. Luria

* As of January 1, 1948.

Section G (Botanical Sciences)

American Society of Plant Physiologists	J. van Overbeek
Mycological Society of America	Lindsay S. Olive
The American Phytopathological Society	K. Starr Chester

Section H (Anthropology)

The American Anthropological Association	Frank H. H. Roberts Marshall T. Newman Weston La Barre

Section K (Social and Economic Sciences)

The American Sociological Society	Alfred McClung Lee Calvert Dedrick Abraham Jaffe

Section M (Engineering)

American Society of Civil Engineers	Allen Wagner
Illuminating Engineering Society	Matthew Luckiesh
Institute of the Aeronautical Sciences	John J. Glennon
The American Society of Mechanical Engineers	L. M. K. Boelter Huber O. Croft Carl F. Kayan W. Julian King Alexander Klemin
The Institute of Radio Engineers	Laurence G. Cumming

[*Continued on page 182*]

BOOK REVIEWS

THE NEW VISUAL ART

Television Techniques. Hoyland Bettinger. ix + 237 pp. Illus. $5.00. Harper. New York. 1947.

NOW that television is no longer "just around the corner," but with increasing receiver production and sales begins to emerge as a medium possessing individuality and distinctive character, it is logical to expect the appearance of such a book as Hoyland Bettinger's. Mr. Bettinger, former general manager of television station WRGB in Schenectady, writes with authority concerning program production problems in television today.

The first third of Mr. Bettinger's book is devoted to indoctrinating the reader to the medium. He describes the technical studio equipment and explains how it is used, dwells briefly on viewing habits of the audience, and discusses the all-important limitations of time, space, light, and sound pickup. He then launches a chapter entitled Pictorial Composition and Continuity, an exposition, with line drawings for illustration, of the basic principles underlying all visual arts. In presenting these basic principles of composition as they apply to television, Bettinger saves the student a good deal of digging and research and supplies the working television producer with excellent reference material. The next two chapters, which complete the indoctrinary portion of the book, cover Video and Audio Techniques, and contain much valuable practical information.

One of television's serious problems has been its failure to produce good creative writing. If Bettinger's chapter on Television Writing is widely read and understood, the problem may be well on its way to solution. He contends that "the television writer . . . is not one who sits in an ivory tower and writes as he pleases," but rather must create with full consideration for (1) the intimate aspect of television as it is received in the home, (2) the technical and production limitations of the studio in which his script will be produced and (3) the interpretive limitations which time and budget force on his producer. Mr. Bettinger goes on to discuss continuity and playwriting, with suggested forms for scenario and production script.

The rest of the book, with the exception of final chapters on the use of film in television and studio lighting, is concerned specifically with the problems of the producer-director—mainly in relation to the production of a play. Although at first glance one might think Bettinger puts disproportionate emphasis on dramatic production in relation to a balanced program schedule, it is true that the problems to be solved in dramatic production for television occur in lesser degree and in different combinations in almost every other type of production.

Television Techniques, then, is a primer, a sort of textbook, which should be of extreme value to the student and at the same time can serve to remind those working in television of the reasons behind the things they do.

FRANCES BUSS
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SOCIOLOGY

The Theory of Social and Economic Organization. Max Weber. A. M. Henderson and Talcott Parsons, Translators. x + 436 pp. $4.00. Oxford Univ. Press. New York. 1947.

IT IS a curious commentary on a country that likes to think of itself as always up to date that the popularity of Toynbee's *Study of History* should date from 1947, whereas his first volumes appeared in 1934, and that Max Weber, the Colossus of Heidelburg, should wait more than a quarter of a century after his death for the translation of essential portions of his work. Many complacent readers feel that anything worth read-

ing gets an English dress sooner or later, but surely this is true only in the very long run.

Now, thanks to the almost simultaneous appearance of Gerth and Mills' *From Max Weber* and the present translation of Part I of *Wirtschaft und Gesellschaft,* both with excellent introductions, excuse for neglect of Max Weber disappears. Still the barrier of mentality is left, and Weber may appear too much the methodologist, may permit himself a ranging over many disciplines that is suspect to the specialist, and may be too exclusively the "technical analyst of social phenomena" to please the factually minded. For the admiring, however—and their number should be increased by Mr. Parsons' meticulous translation and editions—these rare abilities are precisely Weber's claim to a preeminent place among twentieth-century social scientists.

Weber writes with an Aristotelian meatiness that makes it impossible to underline and difficult to select. A reviewer has no choice but to try to select the most essential points.

The procedure he preaches and exemplifies is the formulation of—

> pure ideal types of . . . forms of action which in each case involve the highest possible degree of logical integration by virtue of their complete adequacy in the level of meaning . . . (p. 110). The more sharply and precisely the ideal type has been constructed, thus the more abstract and unrealistic . . . it is, the better it is able to perform its methodological function in formulating the clarification of terminology, and in the formulation of classifications, and of hypotheses (p. 111).

The very subject matter of sociology is an ideal type. For Weber, action is social only when it is "oriented to the behaviour of others." This orientation must be meaningful. This leads him to a comparative neglect of action that is causally determined by the action of others, but not meaningfully. Distinction between the two kinds of social action is of course justifiable, but limitation to one kind is less so—a fact Weber has to admit in the paradox that "sociology . . . is by no means limited to the study of 'social action'" (p. 114).

The temptation to which Weber has yielded in making meaningful social action central to his subject matter is a result of his definition of explanation. If explanation is an emphatic penetration of the subjective meaning of action, then naturally one chooses to study the type of action that has such subjective meaning, even while admitting that it forms but a part of what goes on in human society. Within this narrower field, insistence on *Verstehen* as necessary to render action intelligible is a valuable contribution. Weber rightly confesses that it is only part of the story and that the ability to attribute interest or meaning to an act may give us only plausibility if we do not have in addition some assurance as to probability of the repetition of such acts. Statistics alone is not enough for the student of causality, but neither is an imagined subjective meaning.

Social action may be treated as action oriented to values, as well as toward persons and their opinions. Such values may be absolute and unconditional, pursued without weighing alternatives or costs. Cases of this sort form one extreme limit in the direction of irrationability; the purely rational and calculated is the other extreme, while concrete human action is usually a mixture of the two. One would like to see a fuller discussion of these means and ends, a fuller admission of the fact that even absolute values depend upon the culture of the individual and are subject to the impact of cultural change.

The distinction made here carries over into Weber's discussion of the nature of economic organization, which is essentially rational and calculated, and most so in its capitalistic form. This part of Weber's work, aside from its emphasis on the well-known "Protestant ethic," contains less that is novel.

The treatment of types of authority, with which the volume closes, is wise, closely reasoned, and roams through time and space with magisterial tread.

Authority may rest on rational grounds, which curiously enough are practically equivalent to legal patterns and to the employment of a bureaucratic administrative staff, which consists ideally of those who have the know-how, and which is defended by insisting that the choice is between bureaucracy and dilettantism. Moreover, by opening the way to those who prove their rational and

technical knowledge, it is democratic in tendency, as opposed to the authority of those whose claim rests on the other two grounds of tradition and charisma, the gift of grace. Since followers, and the "people" in general, want results, there is a tendency to replace an ineffective tradition or a failing charisma by a more routine but more rational type of authority, or at least to admit an admixture of the other types of authority better adapted to the conditions of economic life, which are always exerting their pull.

In almost any concrete case, elements of rational, traditional, and charismatic authority will be present. In the Federal government of the United States there is, we hope, rationality, certainly tradition, and some may discern the minimal charisma of a Truman.

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SEISMOLOGY

When the Earth Quakes. James B. Macelwane. xi + 288 pp. Illus. $5.00. Bruce Pub. Milwaukee. 1947.

THIS 288-page book is one of the "Science and Culture Series," of which the Rev. Joseph Husslein is editor. It is written by one of the most distinguished seismologists in the world and is aimed for the most part at the general reader. There are 17 chapters and a geological appendix, as well as a list of reference reading and a glossary of terms. In addition to a general discussion of earthquakes, there are chapters on engineering seismology, seismic prospecting, rock bursts and microseisms. The illustrations are profuse.

A general chapter on What Earthquakes Do is followed by a discussion of eighteen particular shocks. These include a shock (Hawaii, 1868) associated with a volcanic activity. Some 73 illustrations accompany these two chapters, including maps of shaken areas and pictures of damage done.

Chapter 3 is entitled Kinds of Earthquakes. Depth of focus is first considered. The author states that "the great preponderance of strong earthquakes seem to originate at depths between five and twenty miles." These are classed as normal shocks. Those originating at depths much greater than twenty miles are called deep. The classification of tectonic, volcanic, and plutonic (deep focus) is also offered. "Earthquakes that involve a sudden deformation of the earth's crust by faulting or warping are called tectonic." Thus, warping as well as faulting is envisaged as a source. Regarding volcanic earthquakes, the author states, "Earthquakes may be associated with volcanoes in three ways: 1) an earthquake may originate in the neighborhood of an active or a dormant volcano; 2) it may occur simultaneously with an eruption; 3) it may be caused by volcanism." Later, he restricts the term "volcanic earthquakes" to those "caused" by volcanism, so it returns to its old form. How we are to decide whether a given shock is volcanic or tectonic still remains uncertain. Some four particular volcanic earthquakes are discussed in this chapter. At the end of the chapter the writer presses for the term "plutonic" to describe shocks the depth of which is "measured in hundreds of miles."

In the chapter Why Earthquakes? there are listed "geological conditions in a few typical places where earthquakes have occurred." Discussion includes grabens, rifts, thrust zones, ocean troughs, faulting, and warping. Under ultimate causes mentioned are cooling of the earth, isostasy, thermal cycles, migration. The elastic rebound theory is discussed. The geodetic data used by Reid are given in a table. The map (Fig. 112) is mislabeled "The amounts and direction of actual movement in the California earthquake of April 18, 1906." The data on this map are the same as those in Figure 7, *Special Publication 151,* U. S. Coast and Geodetic Survey. The arrows show changes in position of triangulation stations, holding fixed Lospe, Tepusquet, Mount Helena, and Monticello, between surveys made before 1900 and surveys made between 1922 and 1925. According to the elastic rebound theory, these displacements include slow drift (strain) as well as earthquake displacement.

A chapter on the question of earthquake prediction holds out hope of prediction in

time only on the basis of accurate geodetic observation of accumulation of strain in the earth's crust. As to where earthquakes are most likely to occur, more can be said with the statistics accumulated by seismologists.

After a chapter on field study of earthquakes, there is one on seismic sea waves. Figure 125 is most interesting: that of a tunami breaking on a beach. One wonders what happened to the photographer shortly after he snapped the picture. Perhaps the picture was a telephoto. The next chapter on earthquake sounds presents them as somewhat of a mystery. The author considers some earthquake sounds as traveling a large part of their path in the air rather than all entering the air from the ground almost directly under the hearer's feet.

Chapters 9 and 10 discuss seismographs. There are no equations in the whole book! Particularly in Chapter 9, the reviewer feels that the subject is complicated rather than simplified by this omission. One could wish that in the definitions of dynamic and static magnifications of a recording pendulum on page 145 the author had in each case given the reference system from which the displacements mentioned were measured; "displacement of the support" and "displacement of the mass" are relative to different systems.

One is impressed with the large number of pictures of seismographs and the detailed word descriptions, coupled with the lack of diagrams and equations. To the reviewer these descriptions were most informative—he learned details he did not know.

In the next two chapters is a summary of engineering seismology and measurement of artificial vibrations. Then a chapter on rock bursts is followed by one on prospecting. This chapter is well supplied with diagrams. One is interested in the author's use of head wave (after Schmidt's *Kopfwellen*) for that portion of the refracted wave after the ray has emerged from the lower or refracting medium.

The last chapter outlines the development of the use of microseisms to detect storms at sea, one of seismology's latest advances.

The book is an excellent one and well worth the reading by the seismologist as well as the general student for whom it was written. Any views of Father Macelwane are always well considered and based on his vast knowledge and experience in the field of seismology.

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HERPETOLOGY

Amphibians and Reptiles of the Pacific States. Gayle Pickwell. 236 pp. Illus. $4.00. Stanford Univ. Press. 1947.

THIS book supersedes all others treating of the area covered. It is the final word, to date, on the herpetology of Washington, Oregon, and California; a necessity for beginners in an interesting field; and a "must" for the technical student. It is refreshing to find an author so gracious in bestowing credit on his collaborators as Dr. Pickwell proves to be in his Preface and throughout his book.

A chapter each is devoted to a unique running account of the systematic arrangement of the amphibians and reptiles. These chapters are "readable"—interestingly so—for anyone. Next follow two chapters on life habits, of which so little is known or mentioned in the average textbook. All authentic information on homes, food, growth, reproduction, and enemies is interestingly portrayed.

Chapter 6 is devoted to methods of collecting, care in the laboratory, and preserving of speciments.

Eighty-five plates and figures are so lifelike that even a person only vaguely interested in herpetology will enjoy studying them.

The Appendix, besides containing unfailing keys for determining adult specimens, also contains keys for the larvae and eggs of amphibians. This innovation will be appreciated by all who desire to work with amphibia. Dr. Pickwell concludes with an ample Glossary, Bibliography, and Index.

This book will do much to stimulate interest in natural history and a more sympathetic understanding of the lower orders of life, besides kindling a desire for field

study. The end result will amply repay Dr. Pickwell for his years of painstaking work, the credit for which he has so generously shared with those who helped him write this book.

CHAPMAN GRANT

San Diego, California

DICTIONARY

English-Spanish Chemical and Medical Dictionary. Morris Goldberg. 692 pp. $10.00. McGraw-Hill. New York. 1947.

THIS new English-Spanish technical dictionary contains more than 40,000 current terms used in medicine and allied sciences. A work of this nature implies a great deal of effort on the part of the author, a very worthy effort in view of the existing need for works of this kind. Nevertheless, careful review of the Spanish text discloses some unavoidable mistakes: some are typographical errors, and others are due to inexact interpretation. For instance, in the case of the word "bloodshot," the Spanish equivalent is given as *menchado* instead of *manchado;* "cocoa butter" is given as *Theobroma caco* instead of *cacao;* in explaining *hemólisis* it is superfluous to say "disintegration of the red blood cells" when of course these are only found in the blood; the translation of "inbreeding" is given as *consanguinidad,* which means only the result of closely related mating; *verbio* is used instead of *verbo;* "Adam's apple" is not *bocado de Adán* but *nuez de Adán*—to mention only a few of the instances and not discussing some quite debatable definitions as given in the Spanish text for "antigen," "antivirus," "malaria," "crucible," etc. In view of the great value of such a dictionary, a copy of which should be in the hands of every worker or investigator using both these languages, it would be advisable in preparing a new edition to have the Spanish text proofread by qualified physicians and scientists who have a command of the language of Cervantes.

OSCAR VARGAS

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EXTRASENSORY PERCEPTION

The Reach of the Mind. J. B. Rhine. 234 pp. $350. Sloane Associates. New York. 1947.

IN THE opinion of this reviewer, Rhine's latest book on extrasensory perception (telepathy, clairvoyance, precognition) may be summarized by quoting a sentence appearing on page 44: "If we are not extremely vigilant we tend to find merely what we are seeking."

The history of the development of the scientific method is full of examples of errors in observation and recording produced by experimenters whose objectivity was colored by a driving desire to prove a hypothesis. Apparently, the field of psychical research has primarily attracted enthusiatic believers and only secondarily objective observers who have, by scientific training, learned enough about sources of error in human observation to use the standard instrumentation and experimental method devised over the centuries to secure objectivity.

The first chapter of Rhine's book is devoted to a review of the mind-body problem. In it, the author espouses the "psychocentric," or mind-centered, point of view as a point of departure for reviewing again the history of attempts to establish telepathy and clairvoyance as real human capacities.

The new experimental material of this book appears in Chapters 5 and 6, devoted to precognition (seeing into the future) and psychokinesis (the alleged capacity of the "mind" to affect physical objects). In Chapter 10, the author asks the question, Why is not such evidence accepted at once by the scientists? and suggests that fear of the unorthodox is the explanation. This reviewer believes that there is a more pertinent explanation, namely, that these researches have persistently violated the rules of the scientific method to ensure objectivity in the collection of data.

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PSYCHOLOGY

Hypnotism Today. Leslie M. LeCron and Jean Bordeaux. ix + 278 pp. $4.00. Grune & Stratton. New York. 1947.

THE average person does not know much about hypnotism, or he may know nothing at all about it. Yet he is apt to have some very definite notions concerning it. To him, it is mysterious, vaguely tinged with an aura of quackery and of the occult, and, in general, rather anxiety-provoking. Withal, however, he is likely to be interested in the subject and would welcome an opportunity to learn something of it. Unfortunately, the literature of hypnosis is voluminous and full of claims, counterclaims, polemic, and verbiage. What is needed is a sound, careful presentation of the material, which is firmly rooted in the evidence; such a presentation could be recommended to the interested layman. But few if any books on the matter can be so recommended.

Hypnotism Today is a book written in a reasonably nontechnical vein, and it was prepared, in part, to serve the need for a survey of the field. LeCron and Bordeaux are practicing hypnotherapists, and they succeed in removing much of the mystery and magical quality from their subject. Experimental and clinical evidence is cited in abundance, and it is not the authors' fault that clear-cut answers are unavailable to many of the problems that have been investigated. Throughout, the authors emphasize the need for more research, a very healthy emphasis indeed. Their book is recommended by Milton H. Erickson, himself an authority in the field.

I feel that, on the whole, the presentation of the major facts, methods, phenomena, and theories of hypnotism is satisfactory. The material could be somewhat better organized than it is, and more space should probably have been given these topics. As it is, one has the impression that this part of the book was written in haste, or else that too much has been crammed into a very limited space. The naive reader may have some difficulty in following the account for this reason.

The second half of the book deals with hypnotherapy. This was written, apparently, because the writers recognize the need for brief forms of psychotherapy, and they hope that their discussion of hypnotherapy will aid in showing its value as a means of brief psychotherapy. One can do nothing but support their aim and their hope, but I feel that the whole book has suffered because of the amount of material it encompasses. The presentation of hypnotism has the shortcomings outlined above probably because the account of hypnotherapy required space which might otherwise have been devoted to it.

The presentation of hypnotherapy, in the reviewer's opinion, is not so satisfactory as the account of hypnosis. So much is covered —abnormal psychology, psychotherapeutic methods, hypnotherapy itself—that each topic is treated very briefly. Technical points are raised and controversial issues are debated in this section, which, I am afraid, will leave the inexperienced reader somewhat confused.

One can recommend the first half of the book—on hypnosis—to the general reader. For the second half—hypnotherapy—more preparation is needed than he is apt to have. LeCron and Bordeaux should have doubled the length of their book—or, better still, have written two books, one on hypnotism and one on hypnotherapy.

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TOTAL CONCEPT

Personality. Gardner Murphy. 999 pp. $7.50. Harper. New York. 1947.

LIKE a general making an all-out assault upon a military objective, so Gardner Murphy here attacks, with multipronged approaches and techniques, the problem of personality and lays siege to it through 999 pages. The resulting study of personality, its development, structure, and meaning, provides one of the most comprehensive treatments of this problem yet to appear.

Dr. Murphy's approach to personality is made, he explains, "chiefly in terms of origins and modes of development on the one hand, interrelations or structural problems on the other." Recognizing the impossibility of

doing justice at the same time to "the quantitative problems revealed by psychometrics, by factor analysis, by ratings, and by questionnaires, or to personality tests or therapeutic and educational problems," he chose to formulate a working conception of personality rather than to define in detail the infinite variability of personalities. Materials referring to diagnosis, to therapy, or other clinical approaches to a specific personality are, therefore, excluded. The understanding of a specific personality can best be achieved, Murphy believes, through a general consideration of personality as such, much as an understanding of the peculiarities of a particular radio proceed from a general knowledge of radios. Thus he has fashioned his book into a *general* psychology of personality.

The leitmotif by which he unifies the wealth of material presented is the view that "every aspect of personality is conceived in terms of the molding, the complication, the interpretation, the concealment, the indirect expression of motives." Beginning with the biology of motivation and proceeding, through a consideration of the learning process (which itself shows dependent relationships to needs), to more complex human motives, and to the symbols, values, and goals that serve as a measure of personality, Dr. Murphy considers such topics as the development of perception, of memory, of imagination, of creativeness (genius); the origins and development of the self; personality structure and personality deviations; and the role of social and cultural determinants of personality.

The scope of this book cannot be indicated, however, by an enumeration of topics, for Dr. Murphy's erudite presentation involves the coordination of data from such fields as anthropology, sociology, biology (genetics, embryology, physiology), education, history, literature, and psychology. In addition, he evaluates and indicates the relationship between experimental, biographical, and clinical findings. Because of this interrelating of various fields the book should prove of value not only to psychologists, but to anthropologists, sociologists, and any others interested in human behavior. It should do much to assist in the cross-fertilization of the various disciplines concerned with the behavior of man.

In the interest of readability, no documentation occurs in the text. All references appear in a separate Index, referring back to page, paragraph, and line. The book has a 749-item Bibliography and a 20-page Glossary.

Dr. Murphy is chairman of the Department of Psychology at the College of the City of New York and editor of the psychological textbooks published by Harper and Brothers.

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SCIENCE ON THE MARCH

Science in Progress. George A. Baitsell, Ed. xv + 353 pp. Illus. $5.00. Yale Univ. Press. New Haven. 1947.

THE Sigma Xi National Lectureships which, with an introduction by Frank B. Jewett, comprise this fifth series of *Science in Progress,* are rather unique among general reviews of contemporary science. The record of advance is described here with clarity and authority, but without concession to the conception of science that pervades most popular reviews. Furthermore, these essays are not restricted to recent experimental results, for science is also presented in progress with respect to the emerging interdisciplinary ideas and tools that will determine experimental emphases for decades.

Dr. Jewett, former president of the National Academy of Sciences, introduces this volume by considering some "broad underlying factors in social affairs, which are destined to influence profoundly the future of all scientific research in the postwar years." His point of view is catholic, and the problems he weighs include the support of science by government, the effect of the war and the atom bomb, and the present dearth of scientific manpower.

In the first review paper, "The Interior of the Earth," James B. Macelwane lucidly reports seismological evidence indicating that the earth consists of "a layered crust underlain by a shell some 600 miles thick, under this an intermediate shell some 1,100 miles in

thickness, and at the center a core or nucleus with a little more than half the diameter of the earth." In the following paper, "Development of the Betatron and Application of High-Energy Radiation," Donald W. Kerst tells how the idea of accelerating electrons by magnetic induction gradually developed over a period of twenty-five years and reached a fruition in the betatron and in the applications of the betatron to therapy, metallurgy, and fundamental physics. Hugh S. Taylor then presents the evidence that led scientists in 1940 to "minimize all emphasis on distortion of crystals, abnormal dimensions of lattices, extra-lattice aggregates of atoms with abnormal activity" and regard instead "the catalyst surface as nothing other than the normal faces of particular catalytic crystals." This article, "Contact Catalysis Between Two World Wars," also contains a record of major technical achievements in the period 1919–45.

The fourth paper, "Fundamentals of Oxidation and Respiration," by L. Michaelis, is an exposition in a stimulating style of the postulate that, in oxidation-reduction reactions, "only a single electron can be transferred at a time and that any bivalent oxidation consists of two successive univalent oxidations." Michael Heidelberger then reviews the present understanding of complement, "its intensifying and reinforcing action on some of the immune mechanisms, its important function as the drudge of the diagnostic laboratory, and something of its curious chemical complexity and extraordinary lability."

Within a relatively short period the general problem of the mechanism of inheritance has become accessible to biochemical investigation. In his article, "Genes and the Chemistry of the Organism," G. W. Beadle presents experimental results bearing upon this development and shows why it is now reasonable to regard single genes as inheritable controllers of single enzymic reactions. The article by Peyton Rous, "Concerning the Cancer Problem," is an exceptionally interesting assembly of evidence related to carcinogenesis and concludes with a critical analysis of current hypotheses.

"Mutation and hybridization proceed on a colossal scale among plant pathogens." In his paper, "Plant Diseases are Shifty Enemies," E. C. Stakman explores the important practical consequences of these phenomena. C. C. Speidel shows some "of the characteristic activities of cells, recorded directly from living animals by fast motion cine-photomicrography" in his account of "Living Cells in Action." The last paper, "Recent Advances in our Knowledge of the Anterior Pituitary Hormones," is by Herbert M. Evans. This article describes the discoveries that followed the development of nonfatal procedures for surgical removal of the anterior pituitary body. These include the biological, physical, and chemical characterizations of four pure, or purified, proteins from this source, having hormonal activity.

Without exception, the authors of this volume have documented and explained their complex topics in direct and scientifically honest terms. Each article succeeds in evoking an active interest in its subject, and the fifth series of Lectureships holds in every way to the remarkable standard set by the previous volumes.

Howard S. Mason
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NOMENCLATURE PROBLEMS

The Geographical Names of Antarctica. 253 pp. Free. U. S. Board on Geographical Names, Dept. of the Interior. Washington. 1947.

OF ALL the areas in which confusion prevails over the spelling and proper use of geographical names, the polar regions, perhaps, show the greatest variation. In its Special Publication No. 86, the U. S. Board on Geographical Names deals with some of the problems of Antarctica. For instance, in many cases where explorers, in an excess of modesty, have avoided giving their own names to geographical features and have attached names of close relatives, the B.G.N. has thought it preferable to drop given names and thus honor the actual discoverer.

Appropriate generic terms have been assigned, and the B.G.N. has tried to find geographical delimitations. For example, the

name "Marie Byrd Land" has been extended to apply to the whole icecap and its coasts east of Ross Sea and as far as it seems to retain the same character; i.e., to Eights Coast. In the interior, the name "Ellsworth Highland" has been given to the strip along the route of Ellsworth's trans-Antarctic flight, replacing "James W. Ellsworth Land." The latter had been artificially delimited by meridians, and comprised much unexplored land; it included also areas now under the designation "Marie Byrd Land."

We cannot expect immediate or general acceptance of such changes, even though they are based on sound geographical principles. We consider it, for instance, a well-founded suggestion to name the eastern and western part of the coast between 74° E. and 59° 40′ E. Mac-Robertson Coast and Lars Christensen Coast, respectively, with Cap Bjerkö a well-chosen dividing point between them. But why should Australians and Norwegians accept this decision? For that matter, when we *decreed* that Graham Land was henceforth to be Palmer Peninsula, the Chileans adopted not our objective, but our method, and *they* decreed that the region should be renamed in honor of O'Higgins; the Argentinians are about to follow suit with still another name.

It seems to me, also, that the wisdom of now translating everything into English is questionable. "Guardian" Islands may be the translation of Øygarden, the name given by the Norwegian explorer, but it will hardly be recognized as such by everyone. If other countries follow the same policy, the confusion will soon be greater than ever. It might have been better to *recommend* the use of such names both in English and in the language of the explorer.

The work on these antarctic names has resulted in an incidental, but for some users an even more important, contribution; that is, a compilation of a comprehensive list of expeditions. Minor disagreements over just which sealing or whaling expedition ought to be included do not detract from the great service rendered by this compilation.

ERIC FISCHER
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PUBLIC HEALTH—100 YEARS AGO

Duncan of Liverpool. W. M. Frazer. 163 pp. 8*s.* 6*d.* Illus. Hamish Hamilton. London. 1947.

DURING the middle portion of the nineteenth century Liverpool might have been considered a continental Singapore. Its principal activities were concerned with maritime operations, and its population consisted to a great degree of foreigners. It grew rapidly, and housing became scarce, with crowded living conditions and poor sanitation. The people became tough, illiterate, and drunken—an ideal medium for the propagation of the diseases brought into the city from other countries on ships.

In 1847 a new office was created to cope with this situation, and Dr. William Henry Duncan became the medical officer of health for Liverpool, the first public-health officer of this type in England. He was courageous and ambitious enough to create an administrative machine that could carry out sanitary reforms and thus greatly raise the standards of health of the community.

This small volume has been written by the most recent successor to Duncan as medical health officer of Liverpool on the hundredth anniversary of the creation of the post. It is compiled principally from public records and letters and contains almost nothing of the personal life of Duncan. It suffers somewhat from lack of spontaneity and will be of interest chiefly to those who seek information on nineteenth-century social and medical conditions in a cosmopolitan city.

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PRACTICAL HANDBOOK

Birds of Malaysia. Jean Delacour. xiv + 382 pp. Illus. $5.00. Macmillan. New York. 1947.

FROM forty months of bird study, observation, and collecting on seven expeditions to Indo-China, which is northeast of, and adjacent to, Malaysia, and a thorough analysis of the scattered and bulky literature, Captain Delacour has created a practical

handbook of Malaysian birds. The simple keys, short descriptions with numerous figures, geographical distributions and relationships, and the hints on habits make this volume particularly valuable to persons interested in birds but without special technical training. Heretofore, only those familiar with the literature or having personal knowledge of the region have had such information.

Malaysia includes the larger islands of Java, Sumatra, and Borneo, the Malay Peninsula, the Palawan group, and the numerous small islands that surround them. The Malaysian subregion consists of five provinces, Malaya, Sumatra, Borneo, Java, and Palawan. The main features of each are included in this volume largely as quotations from F. N. Chasen's *Handlist of Malaysian Birds*. This 600,000-square-mile area, situated astride the equator and characterized by a diversity of climates and altitudes, possesses one of the richest exhibits of bird life in the world, both in number of species and in their specialization.

Delacour's imposing list of birds, excellently illustrated in line drawings by Earl L. Poole and Alexander Seidel, includes such unusual creatures as the Great Argus, the hornbills, and many less famous but equally interesting avian species. All forms of Malaysian birds (except the sea birds and shore birds) known to be valid are sufficiently described for identification by observation. Anyone particularly interested in these groups is referred to *Birds of the Southwest Pacific*, by Ernst Mayr (Macmillan, 1945), and to *Birds of the Ocean*, by W. B. Alexander (Putman, 1928).

This volume follows the pattern adopted for *Birds of the Philippines* (Macmillan, 1946), written by Delacour in collaboration with Ernst Mayr, and whenever possible numerous passages have been reproduced or adapted to make the use of both works easier.

The English vernacular names are those used by Chasen, except when such names were considered to be misleading. Native names have been avoided, and no attempt has been made to give vernacular names to subspecies. These may be easily distinguished by geographical adjectives. Scientific names have, of course, been employed to express precise identities. Nomenclature is generally that used in Chasen's *Handlist*.

Of particular interest is the brief section on Hints to Observers. The author, recognizing that too little is known of most Malaysian birds, encourages bird students and others to add to existing knowledge of these forms at every opportunity.

Birds of Malaysia, Captain Delacour's second contribution to "The Pacific World Series," abundantly fulfills its author's desire to produce a practical handbook, succinct, but complete and up to date.

E. J. Woolfolk

U. S. Forest Service
Washington, D. C.

COMMENTS AND CRITICISMS

NATIONAL DOG WEEK

Too late for mention in my article on "The Compleat Antivivisectionist," I learned of the establishment of National Dog Week, Inc., to reward the outstanding research contribution of the year to the welfare of dogs (*Science*, 1947, **106**, 389).

This strikes me as a most happy inspiration. Because it is based on sentiment, it may expect to win friends for science who judge by their feelings and who can never be won by the hard facts scientists are accustomed to cite in their own defense. Antivivisectionists are characteristically not swayed by logic but by sentiment. Having said some hard things about the ineptness of scientists in public controversy, I hasten to salute this rare instance of the sympathetic employment of the moral values of the opponent. Let us hope that by meeting them on their own ground the scientists will succeed in disarming and winning their opponents.

HELEN MACGILL HUGHES

Chicago, Illinois

RACIAL INTELLIGENCE

It was with amazement that I read Professor Garrett's dissertation on "Negro-White Differences in Mental Ability in the U. S." (October SM). Quite properly, at the outset, Professor Garrett recognizes that while it is ". . . difficult to obtain a fair comparison of American Negroes and American whites, this can be done if proper precautions are taken. Inferences drawn from many comparisons, however, must of necessity be tentative." With these amenities out of the way, Professor Garrett proceeds utterly without caution to conclusions which are anything but tentative.

Professor Garrett's thesis is that differences in mental ability between Negroes and whites are attributable to race differences and not to environment. (In fact, he adduces that the diminishment of the differences in mental ability is to be accounted for by admixture of white ancestry to the American Negro.) In analyzing McGraw's study, the author states that the subjects of this study were babies ". . . all living in a Southern community . . . the white babies were slightly taller and somewhat heavier than the Negro children. . . . The average Negro parent reported six grades of schooling . . . the average white parent reported high-school training. . . ." To emphasize the equality of environment Professor Garrett adds that ". . . many [Negro parents] reported college and normal school attendance . . . [and] many [white parents] had attended only the elementary school." The author then concludes that "environmental influences were minimized, if not eliminated." Would that society could minimize the influence of environment on the Negro as easily as does Dr. Garrett!

I am sure that the author recognizes that even if the duration of school attendance were equal for Negroes and whites, the equality would end there. Does Professor Garrett believe that the influence of environment is only that of school attendance? Can his analysis be considered a scientific recognition of the effects of environment?

Later in his paper, after showing that Northern Negroes scored higher on an Army test than Southern whites, the author argues that ". . . given better schooling the Negro does indeed improve his score—but not his position relative to the white"! Relativity was never so obtuse!

An objective appraisal of the environment of Negro and white even in Northern urban areas reveals the gross inequality in housing, employment opportunities, social security, and education, especially higher education. That, as Professor Garrett states, approximately 25 percent of Negroes do better than the average white is testimony to the courage and perseverance of the American Negro in his attempt to achieve the status of full citizenship despite the many "unnatural" handicaps placed in his way.

Perhaps Professor Garrett's most unfortunate premise, however, is that ". . . Negroes and whites have lived side by side in our country for more than three hundred years." Not only have they not lived side by side, they were not permitted to die side by side in our last two wars for democracy.

DANIEL A. OKUN

Cambridge, Massachusetts

In the October issue, Dr. Henry E. Garrett undertakes to discuss Negro-white differences in mental ability without racial bias; the honest psychologist "does not *care*," says Dr. Garrett, "which race (if any) is the more intelligent."

After this laudable introduction, the nature of Dr. Garrett's unbiased and unprejudiced discussion comes as something of a surprise. On page 332, for instance, he refers to the "shiftless and ne'er-do-well Negro (of presumably low intelligence). . . ." To a Southern plantation owner, this may seem like a scientific description, but I hardly think that the average psychologist would agree, and I may be pardoned for suspecting that a bit of racial bias has crept into Dr. Garrett's science after all.

An examination of his arguments confirms my suspicions. He begins with a 1931 study of Negro and white babies two to eleven months old. "This study is valuable," says Dr. Garrett. Let us consider its value point by point.

1. The sample is small, admits Dr. Garrett. Having made this concession, he blandly ignores it, and draws conclusions as if the sample were large.

2. "Social influences are minimal if not completely absent at these early age levels." Social in-

fluences are now known to have a great effect at these levels. I need mention only one factor, the influence on the infant of direct contact with the mother. Negro mothers are much more frequently forced to leave their homes to support the family, and Negro children suffer from this lack of maternal care.

3. The difference in "D.Q." average was 13 points —trivial, in view of the small sampling, even if other objections are not taken into account. Dr. Garrett draws great conclusions from it.

4. White children had better nutrition, but this, says Dr. Garrett, cannot explain entirely that enormous difference of 13 points. How does Dr. Garrett know? It is impossible at so early an age (compare Gesell, for instance) to differentiate with any sharpness between intelligence, motor ability, memory, etc. Nutrition affects at least motor ability, and therefore the D.Q.

5. Negro and white children react differently to a white psychologist. The importance of the manner of approach to children to be tested, their possible emotional disturbances, and so on are factors now generally recognized by psychologists. Dr. Garrett speaks as if these factors did not exist.

6. The I.Q., D.Q., etc., of children in the first few years of life—and certainly of the first few months —show low correlation with I.Q. results in later life. D.Q. has therefore little to do with Negro-white mental differences.

7. The I.Q. itself is now known to vary with the conditions of the test, the training of the subject, the state of his emotions, his interest in the test, or lack of it, and other factors. An I.Q. test drawn up for American children gives misleading results for British, an I.Q. for city children misleading results for rural, and so on.

It is odd that at this late date Dr. Garrett tries to squeeze support for his statements from the old Army Alpha tests, whose deficiencies are now so generally recognized. Some of the same objections mentioned above apply to them as to the tests on children.

There are other peculiarities in Dr. Garrett's article. Notable among them is the statement that the American Negro "has been exposed to the same manners, customs, and environmental influences (schools, churches, movies, etc.) as the American white." Not to the same schools, certainly, in Dr. Garrett's native Virginia; nor, except for a few individuals, to the same colleges anywhere. Possibly to the same religion, but not to the same churches; sometimes to the same movies, but from different seats—and the environmental influence varies with the seating arrangements.

These factors may seem trivial to some readers. But they affect the entire emotional life of an individual, and hence his intelligence as we know it. When attention is centered on emotional needs, the I.Q. shows significant changes. For instance, Dr. Bernardine Schmidt, working with so-called feebleminded children, has raised I.Q.s tremendously, many children earlier classified as imbeciles or low-grade morons being improved till they came within the normal range. Similar results have been found by others.

In short, for Dr. Garrett's conclusions, I should substitute these:

1. Environment has a tremendous influence on mental ability.

2. Negroes and whites in the United States live in different social and physical environments.

3. Until this factor is fully taken into account, no study of Negro-white mental differences will be worth the SCIENTIFIC MONTHLY paper it is printed on.

JOSEPH SAMACHSON

New York, New York

THE MUDFOG ASSOCIATION

On looking over the "Headquarters Assignments" for the Chicago Meeting in a recent number, I am reminded of Charles Dickens' contribution to the reporting of scientific meetings: his reports on the meetings of the Mudfog Association for the Advancement of Everything.

Admittedly, the wit is a bit heavy and labored; admittedly, Dickens is carried away by his hostility toward the sciences. But the section headings are funny, and the episode of the pug dog closely parallels actual occurrences during my undergraduate struggles to obtain specimens for the study of the anatomy of the cat. His parody of the eager reporter trying to build a sensational story out of nothing indicates that the past hundred years have not brought about any change in this type of activity.

Members of the A.A.A.S. unfamiliar with the *Reports* might be glad to make their acquaintance. They are a pretty good antidote for pompous writing on scientific subjects; they even make one think a bit as to whether a pet research project has real value.

J. D. REICHARD, M.D.

Staten Island, New York

THE MIMA MOUNDS

I read with a great deal of interest the article in the October number by Dr. Scheffer, "The Mystery of the Mima Mounds."

Excepting the gopher theory, I have heard all the other explanations he says have been advanced for the origin of these mounds and some others that he did not mention, including buffalo wallows, beaver mounds, prehistoric elephant wallows, and that they were made by shovel-nosed sharks when the land was submerged. The last theory was advanced by an oil driller who had found shark's teeth in the area around Bakersfield, California, where these mounds cover hundreds of square miles. The gopher theory is new to me, but I think I can disprove it.

First, gophers do not work in a manner that would tend to form such mounds. The University of California found at its cattle experiment station near Fresno that gophers, when fenced off from their natural enemies so that they are unmolested, work

the soil quite evenly, so that each year the mounds of soil they pile up tend to make a sort of summer fallow job of cultivation. This cultivated soil produces a better crop of grass the following year, and hence gophers do not deplete the soil. They cultivate one part of an area one year and another part the next. They do not move the soil or other material toward a central point.

Second, gophers and squirrels will not live in these mound areas, commonly called "hog wallows," if there is any other more suitable terrain, the reason being that these mounds provide their natural enemies, coyotes and foxes, with a good cover for close approach. It is not nature's way—that gophers could have survived for long ages by building good cover for their enemies.

Dr. Scheffer mentions these mounds being found near San Diego, California. This is true. They are found on most of the coastal mesas from San Diego northward into Orange County and southward into Lower California. San Diego County also provides examples of these mounds under different conditions and different terrain. They are found in a number of inland valleys with narrow canyon outlets and at elevations of 200–4,500 feet. El Cajon Valley near San Diego is about 200 feet in elevation; San Marcos Valley, 350 feet; Ramona Valley, 1,500 feet; and Mendenhall Valley, 4,500 feet. All these inland valleys in San Diego County have one thing in common: they have narrowly restricted drainage outlets. The great Central Valley of California has the same characteristics, only on a larger scale.

It is my contention that these mounds are gigantic ripple marks made by deep water flowing slowly out of these valleys through the narrow openings. The same thing can be seen on a small scale in any tidal basin where the outgoing tide leaves mud flats. The size of the ripple marks on these mud flats will be found to vary with the depth of the water and the rate of flow. The deeper the water, the larger the mounds, provided there is a wide expanse of flats where the water begins to flow faster in channels; then the bottom will be smooth or in ridges paralleling the direction of flow.

If we could view the floor of the ocean between Florida and Cuba where the Gulf Stream moves quite swiftly (for an ocean current), then we might see such giant ripple mounds as we now see on dry land. Again, if we could fill any of the valleys mentioned above, or the great Central Valley of California, with muddy water several hundred feet deep and allow that water to drain out through its narrow outlet to the sea, we would get the same sort of mounds we see there today. A scale model of one of these valleys should prove this point. The ripple mounds in these valleys are never found on the steeper slopes or in the channels where the current moved too swiftly but on the gentle slopes and flats where the current was gentle.

I agree with Dr. Scheffer that these mounds may be found on any type of substrata, glacial gravel, basaltic rock, clay, hardpan, etc. This proves that their origin had nothing to do with ground water or drainage. He asks how widespread are these "gopher mounds of the Mima type?" I have not heard of their existence elsewhere in the world, but if I am correct in my theory of their origin, they should be found all over the world wherever the land was suitable in topography for their formation.

ALLAN O. KELLY

Carlsbad, California

The solution to the "mystery of the Mima mounds" proposed by Victor B. Scheffer in the October 1947 issue may indeed seem farfetched to the casual observer of this magnificent microrelief. However, to a soil scientist who has worked intimately with the problem for the past two and one-half years, the rodent theory is not only credible, it seems to be the only possible explanation.

In Merced County, California, in the "hog-wallow" country of the San Joaquin valley is an area of exceedingly ancient hardpan soil known as Redding gravelly loam, covered almost continuously by Mima-type mounds ranging 3–6 feet in height above the intermound level. This area extends over more than 20 square miles, and when one contemplates the fact that there are about 15 mounds per acre, or nearly 10,000 mounds per square mile, the amount of work expended in this one small area is seen to be tremendous. To attribute this colossal earth-moving feat to small rodents certainly appears ridiculous at first glance.

However, closer observation reveals currently active ground squirrels that have mounds as large as 40 feet in diameter and 10 feet in height. And when one considers that this treeless grassland area has teemed with literally millions of rodents for a period certainly extending far back in the Pleistocene era and possibly further, the amount of work required of each rodent shrinks to plausible dimensions. (Some may question the time period mentioned, but the soil described rests upon an almost undissected alluvial terrace plane, which is the oldest and highest of a series of six or seven terrace levels, the most recent of which already has a hardpan soil of advanced age developed thereon.)

The evidence presented by Dr. Scheffer concerning the occurrence of mounds on shallow soils, the lack of orientation (on level areas), the lack of continuous surface drainage, the accumulation of large cobbles in the intermound areas, the mound "roots," have all been observed in this area. However, there are two contradictory bits of information that should be added to the problems yet unsolved.

In the Merced area, the mounds occur very definitely on the slopes of the dissected parts of the terrace plane. Aerial photographs reveal that on the slopes the mounds are oriented into definite almost parallel rows running directly down the slopes. This does not preclude the rodent theory, of course, but, to include this in the rodent theory, the effect of the slope or moisture conditions on "rodent orientation" must be considered.

A more difficult problem, one less reconcilable with the rodent theory, is the occurrence of externally similar mounds upon softly consolidated sediments

of the Sierran andesite period. Here the surface of the softly indurated bedrock follows closely the surface of the mounds, the depth of soil being fairly constant at 8–15 inches and the height of the mounds varying from 24 to 36 inches or more. In this particular instance, rodents do not appear to have played any large role in the mound formation; some form of erosion or weathering process seems to be the only possible explanation for this phenomenon.

However, this is a special case, and in no way should reflect upon Dr. Scheffer's able presentation; the writer concurs heartily with the rodent theory as the explanation for *most* of the Mima mounds, or "hog wallows." I believe it significant that a biologist and a soil scientist should reach almost identical conclusions while viewing the problem from the different points of view.

Congratulations to Dr. Scheffer for his fine contribution to a fascinating problem.

Rodney J. Arkley
Merced, California

NATURAL RESOURCES

A perplexing question may be raised by comparing two statements in the September 1947 issue of The Scientific Monthly.

On page 184 Dr. Ira N. Gabrielson states: "It makes little difference whether it is in terms of tree or crop growth, wildlife or grasslands, fisheries or fur, if the crop taken is so great as to destroy or to deplete the soil and water producing them, it is exploitation, not management. For every person who understood this fact ten years ago, there are thousands now who understand it at least dimly."

On page 208 Mr. P. C. Keith states: ". . . the coal-to-oil process will cause us to reckon our future supplies by the hundreds of years rather than by decades. At that time the cry 'of our dwindling oil reserves' will be heard no more."

No doubt Mr. Keith is correct—but I wish he could tell us why. Of course, it is a great relief for a pressing problem to be removed from the class marked "urgent." But the logic of the process whereby it goes into the class marked "never to be heard of again" remains obscure. A so-called intelligent species might expect to hear cries about dwindling resources of any kind whatsoever until it had solved the problem of living within the supply of energy and materials that the earth can provide on a permanent basis. Such an expectation would be only a minimum. The maximum would extend beyond the history of our planet. Probably we can agree to leave that ultimate problem to our grandchildren several degrees removed. And probably we shall agree, whether wisely or no, to hand on to them the more immediate problem that we create by using for our immediate comfort and luxury all the reserves that might be saved for "a rainy day"—or an ice age.

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MODERN HOUSING

Let me compliment you on publishing the article "Toward a Science of Housing," by Theodore Larson, in the October issue. It brings out in excellent fashion a line of thinking that needs sober analysis.

In a subject so old, so vast, and so continually new, it is perhaps impossible to separate the engineering from the social. However, it is obviously essential to recognize not only the presence of both engineering and social sciences, but that each is present in its fullest and most extensive form. Mr. Larson's approach falls pitifully short of covering the magnificent scope of the subject and is therefore closely allied to the crackpot. What little this article contains of social thinking seems to echo the leftish political concepts of regimentation, while his grasp of engineering is at least somewhat befuddled.

Let me be specific. He begins with the revelation that: "Walls need no longer be inert masses of masonry with no function other than to hold up the roof and provide shelter against a harsh external environment. They can be made to do extraordinary things—radiate warmth or coolness, glow with soft light, move about, perform household chores. . . ."

That Mr. Larson's walls can be trained like seals is no longer newsworthy, but that his type of thinking shrugs off the value of holding up the roof and keeping out the weather and then completely ignores the social and esthetic values of architectural beauty and repose is typical of the hysteria of those who would herd us into enameled trailer camps. I am not a religious man but within the comfortable shelter of my own stone and hewn-beamed house I find a soul satisfaction which could not exist in Model 13-X, unit Number 6327, of the F.O.B. Detroit Corporation's aluminum masterpiece.

Mr. Larson holds up to unthinking scorn the fact that our present houses have a median age of twenty-five years. Conditioned by the advertising of automobiles and other new mechanical gadgets, this hoary age would seem to condemn itself. But has he ever wandered through the gracious rooms of Williamsburg, or Mount Vernon, or any of a thousand other fine old dwellings? Has he not seen even the smallest of many old-time cottages whose inner comfort and outer charm easily outbids much of both contemporary and futuristic so-called housing? By the addition of only accessories—heating, plumbing, and lighting—the old house with basic architectural merit remains a family comfort and a community asset.

To his 14 percent of all houses that need major repairs and to those without modern conveniences could also be added the thousands of dreary shacks devoid of artistic or suitable design, but in this connection it must be noted that their owners seem to be well supplied with eight-cylinder machines for getting away from their uninspired homes. It is a fact that automobile paper is more gilt-edged than first mortgages on houses. These people have made their choice of values, and home is not among the titles on the hit parade. To this group the steel panel

prefab would appeal, provided it could be occupied on weekly payments smaller than those required for a shack or a tenement in the slums.

However, this hope for a lower level of cost does not appear to be warranted. All the prefab plans today attack the shell of the house as their basic premise. They grope for novelty in wall construction. I have before me a detailed itemization of every cost involved in the building of my house and I find that the total of all costs pertaining to the exterior walls, which are sixteen inches thick of masonry, insulation, and interior paneling, is just 18 percent of the total house cost. Frame construction in comparable houses represents only 14 percent. Would a saving of even one-half these figures warrant the complete destruction of a character and beauty universally appreciated? Will ivy cling to, or even a rambler rose cover, the "sanitary baked-on enamel" of walls that any handy man can erect with nothing but a screw driver?

It is true that a house costs much more today, even comparatively, than it did a generation ago. That is largely because it has already ceased to be a simple shell. Bathrooms cost more than privies; wiring, oil burners, and weather stripping are new expenses. But to advance the claim that these desirable features in a single house on a private lot should be made available to the lowest income groups is simply to repeat a hypnotic fantasy. However, these groups do find their modern housing in multiple units. In fact, the apartment house answers the needs of not only low-income families but also of thousands of others who, because of circumstances, do not find the single house expedient. Let the prefab house promoters realize that their costs cannot possibly rival the economies of the well-planned multiple unit.

While it is evident that I find little sense or reason in Mr. Larson's article, I do recognize the underlying need for improvement in housing economics just as in other fields. But I submit that this improvement is a process already under way in a logical manner quite consistent with our civilization. To blame the industry for a housing shortage makes even less sense than to blame General Motors because I do not own a new Cadillac.

The housing industry is not technologically backward. On the contrary, it has made prefabrication and assembly-line production so common that more people than ever aspire to housing luxuries undreamed of a few years ago. Today's carpenter is derided for his lack of skill, but consider that he no longer has the necessity for developing true journeyman status. His doors and windows, carefully packaged with their frames, arrive complete from the assembly line. His cabinets, millwork, flooring, stairs, mantels, and bookshelves are all out of the catalogue. The plumbing, heating, and lighting fixtures are all factory-made. Still larger assemblies are already common, such as kitchen and bathroom units. To deliver at least these two rooms as complete assemblies is but a step and is on the way. This indeed is economic prefabrication.

Were our lending institutions themselves better educated in sound design rather than in quick financing of nondescript and uninspired "housing," our twenty-five-year-old structures would radiate even increased charm instead of the dejection of the slum. After all, it takes twenty-five years to achieve fully landscaped grounds. The depreciation of good houses is more apt to be forced by changed surroundings than physical decrepitude, but as this normal reduction in relative value occurs, these houses retain much that is basically good and become available to those who cannot afford new construction. The average ignorant banker, schooled only in the fine art of foreclosure, lends most of his money on houses to which competent architectural design has never been applied. He thus fosters the blight miscalled obsolescence.

The new type of house that Mr. Larson mentions as a possible goal, one which blends its indoors with outside space, is a delightful objective, but to build even two alike would require two identical landscapes.

Let these charlatans at housing turn their resources toward (1) better design for more of the houses that will be built in the immediate future; and (2) the trend toward prefabrication in sub-assemblies that is already rolling and forget the tin-pan-alley wall sections.

LINDSAY LORD

Falmouth Foreside, Maine

HONEYBEES AND RED CLOVER

As an amateur beekeeper, who is more interested in knowledge of bees than in the delectable honey, I wish to comment on Mr. McAtee's "Cats-to-Clover Chain" (September SM). Undoubtedly, honeybees do pollinate red clover. A few years ago the 30-acre field beside my few hives was in red clover (*Trifolium pratense*). The late summer and fall were very dry that year, and my honeybees found their chief employment among the blooms of the clover. Standing anywhere in the field, I could count from six to twelve workers among the blossoms. Some of these bees were collecting pollen, but the majority seemed to be nectar hunters, since their pollen baskets were empty and their abdomens appeared distended. Slowly during August and September their stores of honey increased, although the smartweed (*Polygonium*) flow of early fall was nearly a failure that year.

This year the same field is in red clover, but my bees are not interested. I walked slowly through the field for a hundred yards today [September 20], but the one honeybee I encountered was working the few scattered flowers of the alsike clover (*T. hybridum*). Yet, other species of insects, such as the bee flies (Bombyliidae), clover butterflies (*Eurymus philodice*), and the monarch butterflies (*Danaus archippus*), are common in the field and give every evidence of satisfaction with the clover's bounty. Probably these insects, for all their abundance, are not enough to pollinate a satisfactory percentage of the clover flowerets. The bumblebee, the universally acknowledged effective, is busily present,

but only in very small numbers. Casually estimating the numbers of these four insects, I think they are about as numerous this year as in former years. Only the honeybee has sufficient numbers to do the job of pollination, and it alone is absent. Why?

I haven't the slightest idea. My apiculturist friend, Dr. Dunham, states: "The response to red clover varies with the genetic background of the plants, their physical condition, the soil types on which they are grown, weather conditions, and many other factors." Many of the conditions affecting my bees are identical, but the weather this past summer has been unusually wet instead of dry. Possibly this is somehow a factor. Yet a visit to my hive shows the bees are not busy. The smartweed-goldenrod flow is over, the asters are not yet in bloom. Although other insects appear to enjoy the red clover, the honeybee will have none of it this fall. Even the alsike and Ladino clovers, which are also in bloom in other fields at no great distance, are only moderately attractive. All summer long my bees have found but little of interest in the abundant clover blooms of several types. The little excess honey they have cured has come from other sources, such as the smartweed-goldenrod complex. Why this dearth of nectar when clover blooms were plentiful?

Darwin, on a later page of the *Origin* returns to the subject of the honeybee and the red clover. His words are interesting in view of McAtee's criticism:

> The tubes of the corolla of the common red and incarnate clovers (Trifolium pratense and incarnatum) do not on a hasty glance appear to differ in length; yet the hive-bee can easily suck the nectar out of the incarnate clover, but not out of the common red clover, which is visited by humble-bees alone; so that whole fields of red clover offer in vain an abundant supply of precious nectar to the hive-bee. That this nectar is much liked by the hive-bee is certain; for I have repeatedly seen, but only in the autumn, many hive-bees sucking the flowers through holes bitten in the vase of the tube by humble-bees. The difference in the length of the corolla in the two kinds of clover, which determines the visits of the hive-bee, must be very trifling; for I have been assured that when red clover has been mown, the flowers of the second crop are somewhat smaller, and that these are visited by many hive-bees. I do not know whether this statement is accurate; nor whether another published statement can be trusted, namely, that the Ligurian bee which is generally considered a mere variety of the common hive-bee, and which freely crosses with it, is able to reach and suck the nectar of the red clover. Thus, in a country where this kind of clover abounded, it might be a great advantage to the hive-bee to have a slightly longer or differently constructed proboscis. On the other hand, as the fertility of this clover absolutely depends on bees visiting the flowers, if humble-bees were to become rare in any country, it might be a great advantage to the plant to have a shorter or more deeply divided corolla, so that the hive-bees should be enabled to suck its flowers. Thus I can understand how a flower and a bee might slowly become, either simultaneously or one after the other, modified and adapted to each other in the most perfect manner, by the continued preservation of all the individuals which presented slight deviations of structure mutually favourable to each other.
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> I am well aware that this doctrine of natural selection, exemplified in the above imaginary instances, is open to the same objections which were first urged against Sir Charles Lyell's noble views. . . .

Clearly, Darwin did not regard the clover-bee situation as necessarily static, for the clover might adapt to honeybee pollination; nor did he regard his words as absolute or final. Rather, he regarded the situation as plastic, his interpretations as imaginative and possible, instead of precise and immutable.

McAtee need not have apologized for reopening the question, for outside Russia few believe Darwin infallible. Many, and among them the writer, admire his truly marvelous degree of inerrancy, and for that reason some of us delight in spotting those few errors to be found in the *Origin*.

Furthermore, questions of considerable economic importance are forcing us to reconsider today the bee-clover relationship, for we find that yields of legume seeds, including red clover, have declined until the situation is acute. Large research programs have been established to discover the reason for these falling yields. It is nearly a hundred years since Darwin found, quite correctly, that 20 heads of insect-pollinated clover yielded 2,290 seeds, whereas 20 protected heads yielded not one. Investigators are seeking ways to obtain consistently those high yields. Prominent in the research program is the relationship of the bee to the clover, for that relationship is not wholly understood, even today. In the absence of sufficient wild insects to pollinate the clover, it is of critical importance that we understand better the erratic behavior of the honeybee. Darwin's observations on the subject are still valid, although his interpretations may fall a little short of perfection.

PAUL D. HARWOOD

Ashland, Ohio

THE BROWNSTONE TOWER

OUR regular readers, we hope, noticed the resumption of Science on the March in the January issue of the SM. This section had been discontinued during 1947 while a new and larger staff of official reporters was being appointed by societies affiliated with the A.A.A.S. The response of officers of the affiliated societies to our request for the appointment of reporters representing these societies was most gratifying. Evidently the officers realized that our proposal would provide an outlet for authentic popular information about significant advances in their sciences. From our point of view the appointment of professional scientists, carefully selected on the basis of their professional standing and ability to write for intelligent laymen, would give us a greater number of factual articles of current significance than we have been receiving, thus increasing the interest and value of the SM for our readers.

The first article by an official reporter appeared in the January issue. It was written by Dr. Charles F. Richter, of the Balch Graduate School of Geological Sciences, California Institute of Technology. Dr. Richter, representing the Seismological Society of America, wrote a truly global account of international recovery in seismology since the war. In the present issue Dr. K. Starr Chester, representing the American Phytopathological Society, and Dr. S. E. Luria, representing the Genetics Society of America, make their appearance. As time goes on more and more sciences, both pure and applied, will be heard from in Science on the March through their official reporters.

Appointment of all reporters has not yet been completed, nor have all reporters who have accepted appointment submitted their first manuscripts. We express the hope here that those societies that have not yet taken action will soon do so and that reporters who have been appointed will submit copy promptly without further word from the editor. On pages 162 and 182 of this issue is a list of present reporters and their respective societies.

The question has been raised by some reporters whether it is necessary for them personally to write all contributions for Science on the March in the fields of science covered by their societies. The answer is "no." A reporter may request a colleague to write an article on a subject that he knows better than the reporter. If that is done, we should want the article to be approved by the reporter and submitted by him to the editor. Of course the reporter not only may, but should, consult his colleagues in the preparation of his own articles, even when he is writing about his own work, for everyone has blind spots toward his own writing. Any manuscript that appears to be suitable for Science on the March and is written and submitted to the editor without the knowledge of a reporter will be referred to the appropriate reporter by the editor. If satisfactory to the reporter, such independent articles may be published. Longer manuscripts intended for publication as principal articles may be referred to reporters for review and criticism when their advice would be helpful to the editor. Also, reporters may help the editor by writing principal articles and suggesting to the editor subjects and possible authors for principal articles.

The editor cannot overemphasize the desirability of writing for the SM in a popular but dignified manner. Articles for the SM are to be read, not studied; and they are to be read largely by those who are not specialists in the subject of the article. Therefore, the customary rules for technical writing do not apply. More background is needed than is required for specialists, technical terms of limited use should be translated or defined, bibliographies are not wanted, mathematical equations are to be avoided. Although lacking the appearance of precision and scholarship, factual articles for the SM may be and should be just as reliable and authentic as the original technical articles from which they may be derived. Our new staff of reporters will bring us closer to that goal.

F. L. CAMPBELL

PHOTOGRAPHY IN SCIENCE

Announcing the Second International Competition

Sponsored by

Smithsonian Institution and THE SCIENTIFIC MONTHLY

IN THE previous issue of THE SCIENTIFIC MONTHLY (January 1948) appeared a report on the First International Photography in Science Competition sponsored by the Smithsonian Institution and THE SCIENTIFIC MONTHLY. Lt. Alexander J. Wedderburn, its author, presided at the judging of the entries in his capacity as associate curator of the Division of Graphic Arts, Smithsonian Institution, and he was in charge of the exhibition of the accepted entries in the Smithsonian during November 1947. Having been pleased by the number and character of the entries and by the attention given to the first salon by scientists and the press, Lt. Wedderburn and the editor of the SM proposed that a second competition and salon be held in 1948. Approval of a second event was given by the Executive Committee of the A.A.A.S. for the SM and by Dr. Alexander Wetmore for the Smithsonian. At the same time Dr. Wetmore reserved exhibition space for the second salon in the gallery on the ground floor of the Natural History Building, U. S. National Museum—a better place for the exhibition than that available in 1947. So the second salon is scheduled for the first three weeks of September 1948, and within that period the Centennial Meeting of the A.A.A.S. will be held in Washington, September 13–17.

Although the first salon met the expectations of the sponsors, the second can and should be improved as a result of experience gained in the first. Unlike a pictorial salon in which subject interest, composition, and photographic technique are predominant factors in judging entries, "Photography in Science" emphasizes the utility of the entries in scientific research. Entries that provide scientific information unattainable except through the use of photography are most appropriate, and if, in addition, novel photographic methods have been devised to get the desired information, still more weight will be given. Therefore, it is important for the judges to be able to distinguish those entries that have an element of scientific and photographic novelty from those that are merely excellent examples of conventional scientific photography. Those who view the pictures later will also want information about the significance of the exhibits. Entrants will therefore be asked to paste on the front of the mount under the print not only a neatly lettered or typed title but also supplemental information on the significance of their entries.

The rules for the first salon stated that "only professional scientists . . . are eligible to enter the exhibition" and that "all prints must be entirely the work of the individual contributor or co-workers." We fear that this rule could have been interpreted too strictly. It was not intended to bar cooperation between scientists and photographers, nor to prevent specialists in photographic research from submitting independent entries. When a proposed entry embodies the ideas and skill of both a scientist and a photographer, it would be appropriate to submit it as a joint contribution. The ordinary operations of developing and printing, which may be done for scientists by photographers, would not seem to require special recognition of the photographer.

In the 1947 rules it was stated that the pictures would be returned to the contributors within two weeks after the close of the Chicago meeting of the A.A.A.S. It was not then anticipated that various institutions would request the privilege of exhibiting the pictures after they had been shown by the Smithsonian and the A.A.A.S. At this writing five such requests have been received. The pictures will be shown elsewhere, and return of the entries to the contributors will therefore be delayed. It will be understood

that all or part of the entries for the second salon may be borrowed for showing in other cities after the close of the exhibition in the Smithsonian on September 21, 1948.

The following men have expressed their willingness to serve as judges of the second competition:

For the physical sciences: Dr. Wallace R. Brode, recently appointed associate director of the National Bureau of Standards. He has done outstanding work in spectroscopy and organic chemistry.

For the biological sciences: Professor Charles T. Brues, Harvard University. A scholar in entomology and related sciences, Professor Brues has made much use of photography in biology.

For the medical sciences: Dr. Edward J. Stieglitz, a physician practicing in Washington, D. C. He is known for his books and articles on gerontology.

For photography: Dr. Sidney S. Jaffe, Washington, D. C., a member of the National Photographic Society.

The foregoing committee will be headed by Lt. A. J. Wedderburn, who again will be in charge of the salon.

Summary of Information and Rules

Purpose. To show examples of the uses of photography in scientific research.

Sections. (1) Black-and-white prints; (2) colored prints and transparencies.

Who are eligible. All scientists everywhere, including those engaged in photographic research. Scientists may collaborate with photographers, and when the photographer's contribution to the final product is substantial, the entry may be submitted as a joint contribution from the scientist and the photographer.

What are eligible. All photographs taken for scientific purposes. Purely pictorial photographs and hand-colored prints are not eligible.

Dates. Entries will be received by the editor of The Scientific Monthly from July 26 to August 16, 1948, inclusive. Entries will be judged on August 21, 1948. Those accepted will be shown in the Natural History Building, U. S. National Museum, Washington, D. C., from September 1 to September 21, 1948. All or some of the pictures may be exhibited later in other cities by institutions requesting the privilege.

Judges. Dr. Wallace R. Brode, Professor Charles T. Brues, Dr. Sidney S. Jaffe, Dr. Edward J. Stieglitz, and Lt. Alexander J. Wedderburn, chairman.

Criteria. The greatest weight will be given to entries that show novelty of application of photography to scientific research and originality of photographic technique. Good examples of conventional photography applied for ordinary scientific purposes will be acceptable, but will be given less weight than those that show scientific originality. Although photographic quality of entries will have the least weight, it should not be neglected.

Awards. There will be first, second, and third awards and five honorable mentions in each of the two sections, black-and-white and color. Awards will take the form of certificates suitable for framing. To each print accepted for exhibition, the label of the Smithsonian Institution will be affixed.

Number and preparation of entries. Each contributor may submit up to eight entries, four in color and four in black-and-white. Each print must be mounted on stiff white cardboard, 16 x 20 inches. One mount may carry more than one print, if necessary to show progressive steps, and each mount will be regarded as a single entry. The dimension of the prints is immaterial, provided space remains on the front of the mount under the print or prints for a neat label giving the following *essential* information: Name, highest degree, official title, and business address of the contributor, including the name of the company or institution employing him; title of the entry and magnification; a concise statement of the scientific significance of the entry. For the benefit of the judges, additional technical data may be given on the back of the mount, and to assist in identification of the entry, the contributor should give its number on the back of the mount; e.g., "This is entry No. 2 of four entries in the black-and-white section."

Color slides or transparencies should not exceed $3\frac{1}{4}$ x $4\frac{1}{4}$ inches and must show the contributor's name and address, and the title

and number of entry. Complete information, as specified above for the mounts, must also be given for the transparencies, but on separate sheets of paper properly identified and attached to the entry form. Slides may be in glass or cardboard, but glass is recommended. Each slide must be spotted in the lower left-hand corner. If not bound, transparencies should be protected in cellophane envelopes.

Shipping and handling. All pictures must be packed flat with sufficient protection. Reinforced cardboard cases designed for mailing 16 x 20 inch mounts may be purchased from photographic supply houses. Shipping labels will be supplied with entry forms. Entries should be sent prepaid to The Editor, The Scientific Monthly, 1515 Massachusetts Ave., N. W., Washington 5, D. C. When the entries have completed their travels, they will be returned to the contributor by express collect.

The receipt of entries will not be acknowledged, but as soon as possible after the judging a card will be mailed to each entrant indicating the entries accepted. Those not accepted will be returned promptly to the sender.

It may be assumed that all exhibitors will handle the entries with care. The Smithsonian Institution and The Scientific Monthly will take pains to prevent damage or loss of entries, but assume no responsibility if such should occur.

Publication: Unless specifically forbidden by the contributor, The Scientific Monthly reserves the right to publish any of the entries accepted, and to grant permission to publish them in newspapers and other periodicals.

Inquiries about the Second Photography in Science Salon and requests for entry blanks should be addressed to The Editor, The Scientific Monthly, 1515 Massachusetts Ave., N.W., Washington 5, D. C. Submission of entries is understood to imply acceptance of all the foregoing conditions of entry. The entry form, properly filled in, must accompany entries.

REPORTERS FOR "SCIENCE ON THE MARCH"—*Continued*

Section N (Medical Sciences)

American Medical Association	Austin Smith
American Psychiatric Association	C. C. Burlingame
American Veterinary Medical Association	R. C. Klussendorf
Society of American Bacteriologists	Monroe D. Eaton Wayne W. Umbreit
The American Physiological Society	John Field

Section O (Agriculture)

American Society for Horticultural Science	H. B. Tukey Ora Smith V. T. Stoutmeyer
American Society of Animal Production	C. F. Huffman
Society of American Foresters	William M. Harlow C. R. Lockard
The American Society of Agronomy	C. E. Marshall H. M. Tysdal